

Unemployment and Health 1975-1987:  
A case study in the relationship between research  
and policy debate

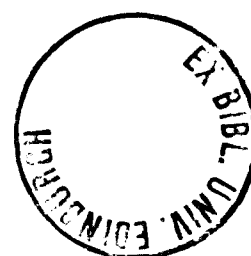
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*In memory of Salka Viertel*



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## ABSTRACT

This study follows the progress of an academic controversy, on the consequences of unemployment for physical health, which took place in Great Britain between 1975 and 1987, treating it as a case study in the relationship between research and policy debate. There are three aims to the study: (1) to document its progress in detail in order to clarify the interactions between its "academic" and its "political" aspects (2) to investigate the extent to which "quality" as defined by the academic community influences the impact of research on policy debate (3) to propose the outline of a model of policy-related academic debate which can be applied to other cases and therefore contribute to the study of the relationship between research and policy.

The study uses a combination of interviewing, documentary research and participant observation, and sets published papers within their context of the formal and informal relationships between researchers, professionals, policy makers, pressure groups and the media. It is guided by a combination of two theoretical perspectives: the models of the construction of social problems proposed by Spector and Kitsuse and Downs, and the "translation" model in the sociology of scientific knowledge drawn from recent work by French scholars such as Latour and Callon. It is concluded that the application of this combination of perspectives leads to a reformulation of questions about the relationship between research and policy. The acceptance of scientific findings appears to depend upon actor-networks which are at least partly constructed as a result of policy-decisions, so that "truth" and "policy-relevance" come to be seen as mutually constitutive.

## CHAPTER 1

### THEORETICAL PERSPECTIVES

#### 1.1 The Debate: A Brief Chronological Account

In mid 1975, an article appeared in *The Financial Times*, written by Lombard columnist C. Gordon Tether, which dealt with the possibility that unemployment might be damaging to health. Eighteen months later, the first conference on the topic took place in Britain, organised by an independent policy unit, the Unit for the Study of Health Policy, and attended by people with a wide range of interests, including social medicine, economics, planning and ecology. Two years later, in January 1979, a World in Action programme "The Reckoning", caused considerable public stir. It, like the Tether article, concerned the work of one particular expert, M. Harvey Brenner of Johns Hopkins University. In the programme, lavishly illustrated by human interest stories from recession-stricken areas, Brenner made claims about the numbers of deaths, mental hospital admissions and prison sentences that would arise as a result of given levels of unemployment.

Even before it was screened, *The Reckoning* had resulted in questions in Parliament. There were more afterwards, and the party in power (Labour, under pressure to adopt deflationary measures by the International Monetary Fund) had to reassure many of its backbench members that it had the situation under consideration. In September 1979, the prestigious British medical journal *The Lancet*, published a paper by Brenner (Brenner 1979) which made an econometric analysis of trends in employment levels and mortality levels in England and Wales, showing that these were significantly correlated in the period 1926–1976.

In the spring of 1981, now with a Conservative government in power, a major conference on unemployment and health was held in Wales, sponsored by the Welsh National party amongst other groups. Brenner was the main speaker, and camera teams had followed him from the USA. In the summer, a report was published by the Department of Health and Social Security on the health of families of unemployed breadwinners (Fagin 1981). The research had been carried out by a psychiatrist, Dr Leonard Fagin. Although only a small

sample, the report detailed many painful effects of unemployment on these families, and received wide press coverage. More Parliamentary questions were asked. More MPs were reassured that the government had the problem in hand, and that Dr Fagin's work had indeed been a "pilot study" to a larger research effort to investigate the effect of unemployment on health.

In September 1981, the fruits of this effort appeared in the academic journals. One paper from the same study as Fagin's, but reporting results for the whole sample rather than a tiny subsample, appeared in *The Employment Gazette* (Ramsden and Smee 1981). Another, a re-analysis of Brenner's macro-economic study of employment and mortality trends in England and Wales, appeared in *The Lancet* (Gravelle, Hutchinson and Stern 1981). Both papers concluded that if there were some effect of unemployment on morbidity and mortality, it could not be demonstrated in either of these data sets, and the hypothesis that unemployment was bad for health could not be regarded as having received any supporting evidence. So that another "Celtic Fringe" conference on unemployment and health, organized and sponsored by the Scottish National Party, held in Glasgow in November, had rather less impact, despite the presence of Brenner once again as main speaker, and enthusiastic promotion by leading political, academic and medical figures in Scotland.

In June of 1982 two further reports of research on unemployment and health emerged. One was from a large-scale study of the health of middle aged men (Cook et al 1982). The other was from a one percent sample of the 1971 Census of England and Wales, followed forward to subsequent vital events and censuses (Fox and Goldblatt 1982). Both in their different ways showed some evidence of poorer health or higher mortality risk amongst people who had reported themselves as unemployed. This time, however, there was little press attention and no questions in parliament.

Questions were, however, asked about the risk of suicide and attempted suicide amongst the unemployed, as a result of the dramatic act of two young boys who, in the autumn of 1982, killed themselves by running a hose from the exhaust of their parents car, leaving a note to say they had been driven to it by the hopelessness of unemployment. This was perhaps the most striking of such incidents, but frequent similar reports in appeared in the media throughout 1982. When research from a prestigious unit in Edinburgh dealing with unemployment and suicide (both "completed" and "attempted") began to

be publicized in 1982, it reached a ready audience (see Platt 1982, 1983), as did further work on attempted suicide and other aspects of the mental health of the unemployed which followed in subsequent years (for example, Banks and Jackson 1982, Gillies et. al. 1985, Warr and Payne 1983, Platt and Kreitman 1984, 1985).

By the summer of 1984, Conservative Health Minister Kenneth Clark had admitted to the annual conference of MIND, the National Association for Mental Health, that undoubtedly unemployment was harmful to health. When further results of the one percent sample study (Moser et. al. 1984) were leaked to the press in September 1984, it aroused (relatively) rather less media interest.

Between 1984 and 1987, studies continued to be published which added to the evidence that there was (Beale and Nethercott 1985, 1986a-d, 1987), or was not (Forbes and Macgregor 1984, Alderson 1985) a cause for concern. And yet there was little further attempt to use this evidence in debates on government economic policies. Nor was debate between the different sides of the academic argument explicitly re-joined, despite the continued appearance of papers critical of Brenner and presenting material which showed no relationship between "unemployment" and various measures of "health" (Wagstaff 1986, Forbes and Macgregor 1987, Charlton et. al. 1987).

The issue never became (during the period 1984-87) as "hot" as it had been in 1979-81. It made far fewer appearances in the media. There were no more large conferences with television teams from the USA, as there had been in early 1981. Headlines of "Death on the Dole" were now confined to local newspapers rather than the nationals. The Labour Party made sporadic use of the idea that unemployment was harmful to health. But when, in January 1987, the very latest (and highest) estimate drawn from a large and academically respectable study using official statistics appeared in the BMJ (Moser et al 1987), it received only a single mention in the media. The issue seemed to have lost its characteristic of being, or being seen as, one which was of major concern to politicians or to the public at large.

This may seem rather paradoxical -- that as the evidence of the existence of a social problem seemed to grow, and as its existence tended to be accepted in certain academic circles and amongst some opinion leaders and policy makers, the salience of the problem, both as a "scientific" and as a

public issue seemed to decrease. Furthermore, although at one point, the media did treat the health of the unemployed as if it should be of concern to the reading and viewing public, at no time did trade unions or groups of the unemployed or those working to advance their interests take it up<sup>1</sup>. The question was treated in academic journals and conferences in contradictory ways: some papers and speeches seeming to accept unquestioningly that unemployment was bad for health, others continuing to treat it as an open question.

Research on the effect of unemployment on psychological health was also carried out and reported during 1977–1987. It will not be discussed in any detail in the present study for two reasons

1. it was not given any priority by the media (with the exception of Fagin and Little's study, which was on the borderline between mental and "physical" health)
2. and possibly the reason for (1), psychological ill-health is not the same sort of phenomenon for political purposes as "physical" ill-health.

The major reason for (2) above is that the question of "physical deterioration" has a long history in British social policy debate (Pfautz 1967, Hennock 1976). It has formed the centre of many political storms over the adequacy of both wages and levels of benefit. The question of whether the notion of giving to someone whose labour is not "in demand", sufficient money to allow "physical subsistence", can be justified within a strict market economy, is an "essentially contested" one in western industrial societies. This problem is compounded by repeated findings of studies since the early twentieth century that the lower levels of WAGES tended to be lower than the amount needed to pay for a diet defined in "expert" studies as sufficient to sustain physical health. In the 1970's and 1980' this problem surfaced in the form of debates over the "replacement ratio" (of benefits to wages, see Micklewright 1986, Cooke 1988), and in the form of "scrounger scares" (Golding and Middleton 1982, Deacon 1976, 1978, Deacon and Sinfield 1977, Popay 1977). purporting to show that benefits were too high, and were used to support deviant or luxurious lifestyles. There was one study, the DHSS Cohort Study, whose results figured importantly in the debate on health, whose major purpose was to deal with the

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<sup>1</sup> The Unemployment Unit carried two articles and one short piece on unemployment and health during the period 1982–1985. The first of these (Platt 1982), attributed high rates of suicidal behaviour amongst the unemployed to pre-existing psychological vulnerability.

question of the "replacement ratio", not primarily in relation to health, but rather in relation to public concern about what some saw as an excessively high level of benefits (see Moylan, Millar and Davies 1984 for a full report of the study's findings).

Suicide and attempted suicide (seen as possible "effects" of unemployment) provided a half-way phenomenon. Insofar as unemployment was being claimed to cause suicidal behaviour via individual "depression", research claims that "unemployment causes mortality" were not fiercely contested. The "alleviation" of psychological consequences was something which officials offered to academics as a "suitable" topic (see section 7.4 below). There were, however, other claims made in the media, to the effect that suicide was a decision taken "while of sound mind" by, for example, young people facing the reality of life without work. These claims drew considerable attention, from the media and in parliament, and also from officials and researchers (see section 8.1.2). The following account will therefore concentrate on the fate of claims that the "unemployed" are subject to risks to their "physical" health, and/or at greater risk of death than the "employed".

## **1.2 A social-problems approach to the relationship between research and policy debate**

F M Martin (1977) has remarked that

There may be a doctoral thesis to be won by sieving through the products of academic social medicine in order to capture a few specimens of policy-oriented research, and by painfully reconstructing the relevant policy processes ...

There have been many attempts to discover how research "influences policy" (for example, Caplan 1976, Rein 1980 and 1983, Weiss 1979, Bulmer 1983, and the various papers collected in Kallen et. al 1982., to give but a few recent examples) how researchers might increase the influence of their work, and what kind of research is taken most seriously by policy-makers. The present study treats the debate on unemployment and "physical" health as a case study in "the relationship between research and policy". It attempts to "reconstruct" and make sense of the progress of the debate in order to address some of the questions posed by previous work on the relationship between scientific and policy debates, and to throw light on the apparent paradoxes to which arose as



the debate unfolded. In pursuit of these aims, the study will document the interactions between the "public" and the "scientific" aspects of work on the question between 1975 and 1987. Tracing these interactions seems to produce an alternative set of questions about the "influence of research on policy", which begins to appear mutual rather than unidirectional.

The theoretical approach adopted will combine two elements. Firstly, the "social problems perspective" as described by Spector and Kitsuse (1977), and as developed by Manning (1985). Secondly, the approach to scientific controversy adopted by the "strong programme" of the sociology of scientific knowledge (SSK). This chapter will outline these two perspectives. The second chapter will go into more detail of how researchers in the sociology of scientific knowledge have applied certain "principles" and "rules of method" to the study of scientific controversies. Chapter 3 will give a brief account of the formation of the social groups which played major parts in the unemployment and health debate, and the background of institutional change against which it took place. Chapters 4 to 9 are arranged in the form of the "stages" of a social-problem process. Within each chapter, sections and subsections both give an account of the major events in the debate, and offer examples which illustrate the social processes involved at the level of the smaller groups of professionals, researchers, activists, journalists and officials, in interaction with each other. Chapter 10 looks back over this account, assesses the extent to which the theoretical approaches adopted have enabled the debate to be understood, and suggests an alternative way of examining future cases in which "the relationship between research and policy" is at issue.

Spector and Kitsuse (1977) have described their formulation of the "social-problems perspective" as placing "the interaction between claims-making groups and others regarding the definition of social conditions and what should be done about them" at the centre of interest. A primary task of the researcher is to examine how some situation or condition is asserted to be morally objectionable, and how collective activity is organised around these definitions and assertions. Social problems are not in themselves CONDITIONS, but rather the OUTCOME of activities, which Spector and Kitsuse term "claims-making" (p. 72-73).

The central problem of a theory of social problems is to account for the emergence, nature, and maintenance of claims-making and responding activities" (p. 76)

The social problem analyst is not concerned with whether or not condition "x" even exists, let alone with whether it is in some final sense "truly" morally objectionable. Rather, s/he must analyze the discourse on the existence of the problem as "factual claims-making" and the moral discourse as "value claims-making". Factual and value claims are interwoven throughout the career of the problem, and Spector and Kitsuse propose four "phases" in which this career can be divided:

Stage One is that in which a group or groups point to the existence of some condition (a factual claim), indicate that it is undesirable in some way (a value claim), and attempt to promote it to a higher position on the agenda of public and political debate.

In Stage Two, some official organisation or institution recognises the "truth" of the knowledge-claim, or at least begins an official investigation to clarify these claims, and begins to formulate an official "policy response".

In Stage Three the original groups declare themselves unsatisfied with official response, and the "policy failure" or "cover-up" becomes a new problem claim.

In Stage Four, claims-making groups abandon their attempt to satisfy their grievances and/or to resolve the asserted problem through official channels and begin to develop "alternative, parallel, or counter-institutions".

This "stages model" is not put forward by Spector and Kitsuse as anything more than a working hypothesis. They do not assume that social problems have a "natural history", let alone the specific one which they have outlined. They hope, however, that eventually:

The original counsel of the natural history concept to examine sequences of events and to document unfolding lines of activities will have produced a rich literature ... a hypothetical natural history may serve as a temporary procedural manual, a checklist of things to attend to, and a first order of business" (p. 158)

The procedure advocated by Spector and Kitsuse is to investigate the claims-making strategies of all the groups engaged in a social problem process, which may include a wide variety of professions, pressure groups, "moral crusaders", official agencies and members of the media. An essential part of this investigation is to "ascertain how PARTICIPANTS in an activity define that activity" (p. 79).

A critical elaboration of this approach as it can be applied to the British policy process is offered by Manning (1985). Like Spector and Kitsuse, he takes a "developmental" approach, and focuses on the process of claims-making rather than on the questions of "whether the problem really exists". This is not a "natural" but rather a "social history". To Spector and Kitsuse's stages he adds a possible "loop" back from III to II (an example is the setting up of a Royal Commission) and observes that "Group claims can get stuck in this loop and disappear" (p. 9-10). He also adds the concepts of "individualisation" of social problems (which I will call "moral fragmentation") and of defining certain problem areas as exclusively the domain of "experts" (which I will call "technical fragmentation"), as processes which accompany the progress of problem-claims through these stages.

His criticism is that the model does not go into sufficient detail about the ways in which claims are put forward by some groups and accepted by others, nor about what determines the way "official" and government agencies react. One reason for this failing is that Spector and Kitsuse have no analysis of the role of the media in the social problem process.

In order to fill this gap, I have followed Oscar Gandy's (1982) analysis of the relationship between experts and the media, and, more specifically, I have made extensive use of his concept of "information subsidy".

Gandy's work applies only secondarily to professional or scientific groups, but forms an extremely useful model for the more public phases of the unemployment and health debate. He points out:

Politicians, bureaucrats, producers and consumers will each attempt to influence the identification of social problems, the formulation of policy options in response to those problems ... They will attempt to produce that influence ... most frequently through the provision of information subsidy to other policy actors...(p.55)

Work on pressure groups and the media, such as that of Oscar Gandy, Field (1982) and Banting (1979) demonstrates that pressure group activity often involves the making of "knowledge-claims" as a central concern. These authors point out that "scientists" and "policy actors" (both those who wish to change the status quo and those who wish to preserve it) all make use of public controversy, often "creating" such debate through that form of skilful use

of the media, which Gandy describes as “information subsidy”. As he puts it,

The information is characterised as a SUBSIDY because the source of that information causes it to be made available at something less than the cost a user would face in the absence of a subsidy. ... That is, the delivery of an information subsidy through the news media may involve an effort that reduces the cost of producing news faced by a reporter, journalist or editor (p. 61–62).

Carole Weiss, who has written extensively on the question of how information is used to influence policy-makers (for example, Weiss 1982), notes that:

generalisations and ideas from numbers of studies come into currency ... through articles in academic journals, journals of opinion, stories in the media ... lobbying by special interest groups, conversations of colleagues, attendance at conferences ... and other uncatalogued sources. (p.290)

Adopting the phrase originated by Shils (1961), she describes this as the “enlightenment model” (a phrase first coined by Shils, 1961) of the relationship between research and policy. Due to the important role played by the media in the “networks of enlightenment” described by Weiss, certain important agenda-setting decisions are taken by the journalists who control claims-making groups’ access to newspaper space and airtime. These decisions take the form of concepts of “newsworthiness” which are well described by various writers on the media (such as Goldenberg 1975, Altheide and Johnson 1980).

Gandy explains that:

Much of the hegemonic process is automatic, unconscious, and part of the “normal” practice of professional journalists.(p.57)

Faced with time constraints, and the need to produce stories that will win publication, journalists will attend to, and make use of, subsidised information ... By reducing the costs faced by journalists in satisfying organisational requirements, the subsidy giver increases the probability that the subsidised information will be used. (p.62)

How can interest-groups thus “reduce the costs” to a journalist of using their information? This question was itself a recurring topic for participants in the unemployment and health debate. Activists and (a few) researchers learned

how to write a press release so as to hit the right balance between presentation of qualified "factual" information and "sensationalism". They strove to guess what would carry "authority" (one of the first things some journalists wanted to know of the provider of a subsidy was "Is s/he a DOCTOR?"). They discovered the value of making something appear to be a "leak". Some even learned the journalists' stylistic rule of writing a press release as an inverted pyramid, with the bits you didn't mind being cut left to the end. All these considerations are enumerated by Gandy, although in his case studies "authority" of the source seems to have been more important than in the present study, and "leak" status less important.

Gandy points out that pressure groups and entrepreneurial subprofessions often act as "expert" providers of "subsidised information" to the media in order to put pressure on political administrators ("the State"), and bring about or prevent policy change. Manning devotes more attention than Spector and Kitsuse to the nature of "the State". He sees the state as a "site of conflicts and struggle" over the allocation of goods and services. Claims upon the state for goods and services are made by pressure groups of various kinds, and here the analysis of J.J. Richardson and A.G. Jordan (1979) is a useful addition to the social problem model. Richardson and Jordan go so far as to argue that, in the course of social-problem processes,

There is a breaking-down of conceptual distinctions between government, agencies and pressure groups; an interpenetration of departmental and client groups. (p. 44)

They contend, and it is borne out by the present case study, that officials and pressure groups are to some extent symbiotic. They give examples such as the Third London Airport and the Channel Tunnel in which proposals were developed jointly by Ministry personnel and members of their "client" groups. Ham (1986) also finds this model useful in analysing the policy process within the Departments of Health and Health Authorities. One reason for this "symbiotic" relationship between officials and pressure groups, which is not spelt out explicitly by Richardson and Jordan, but which was spoken of in some of my interviews, is the need for an official wishing to carve out a "career", to be seen to initiate and/or promote a successful programme or innovation. Contact with pressure groups and "dissident" academics can be a good source of "bright ideas" which, when used with skill and discretion, benefit officials' "cycles of credibility" (see section 1.4).

Richardson and Jordan offer their own version of the "stages model" of a social problem process, based on that of Downs. In this model, more prominence is given (than that accorded in Spector and Kitsuse's version) to two additional "stages":

- 1) the "dramatic event" which alerts the public to the issue
- 2) the "decline of public interest" which sets in once the high cost of "solving" the problem has been realised (p. 90)

They point out that process (2) may be "helped along" by the promotion of new issues onto the agenda of public debate, pushing out the old (from Downs, 1973). They dismiss the idea that the policy agenda is set by pressure groups and interest groups openly lobbying the legislature:

Campaigns are the currency of unsuccessful groups; permanent relationships are the mode of the successful (p. 123).

The reason these "permanent relationships" are given little prominence in the literature on policy making is that their confidentiality is one of their essential characteristics. Richardson and Jordan admit that if the presence of a strong pressure group is combined with Parliamentary interest and good media coverage, policy actions CAN be brought about by "outsiders", but they feel that this is the exception rather than the rule.

Relating his discussion of pressure groups to that of the media, Manning points out that pressure groups have usually

developed an intimate relationship with the mass media [and that] ... Out of this relationship, particularly the exercise of journalistic routines ... comes an image amongst 'informed' opinion as to how grievances should be perceived and whether the resulting claims are legitimate (p. 19)

The media alone, however, cannot create major social problems. In order for this to happen, larger "power blocs" must become involved, such as the business community, the Women's Movement, the Labour or Ecology movements, or alliances of professional groups. Pressure groups on single issues must, therefore, gain the support of these wider constituencies -- McCarthy (1986), using Downs' model, has provided a detailed account of the attempts by the Child Poverty Action Group to enrol more powerful groups in its cause. Other recent examples include the patient lobbying of the trade unions by the Low Pay Unit in their campaign for a minimum wage, and the

activities of the London Food Commission around the questions of food additives and irradiation. A social-problem analysis therefore needs to describe the ways in which small pressure groups seek to have their claims taken up by more powerful bodies.

### **1.3 Government, science and the professions in Victorian Britain – some insights from social history**

Manning's analysis of pressure groups and "the state" however, rather tends to sketch over the internal complexity of government. He observes that the fate of an issue, even one which has gained wide support, depends on "prevailing political priorities" and "departmental politics". The outcome of this is, he feels, to convert social problems into "technical problems related to individuals" (i.e., moral and technical fragmentation). There are several authors who have investigated the internal processes of political administration in more detail. Heclo and Wildavsky's (1974) work on ministers and higher civil servants in Whitehall, and the fate of the Central Policy Review Staff, was pioneering, but deals with a level of government above that at which most of the activity in the unemployment and health debate took place.

Another useful analysis, which deals with a different historical period, is that of Macdonagh (1958, 1961), McLeod (1967) Novak (1972) on the process of government growth and the role which science played in it in the early nineteenth century. Macdonagh (1958) proposes another variant of the "stages model". Like Spector and Kitsuse's, Macdonagh's first stage is "exposure of a social evil", but this model then jumps to the stage of "legislation" rather than dwelling further on the claiming and counter-claiming process involving pressure groups and political parties. Macdonagh's third stage is the discovery that the first legislation is insufficient, and the implementation of stronger measures. Up to this point, the two versions of the stages in the social problem process are quite different, and seem to be applicable to case studies of altogether different types. In Macdonagh's fourth stage, however, he deals with the effect of successful social-problem claims-making upon the organisation of occupational groups, and their relationship to the state. At this stage, a corps of "officers" or "inspectors" of one kind or another are appointed to see that the legislation is implemented. In stage Five, the officers or inspectors develop their own entrepreneurial aims and interests as an

emergent professional group, which helps the process along to Stage Six in which yet more legislation is passed and a central authority is established.

Novak's contribution is to point out that the "expertise" of the corps of inspectors or officers is not something which should be accepted or taken for granted by the social historian. Rather, entrepreneurial groups enter this process as makers of knowledge-CLAIMS, deliberately targetted towards attaining a role in what was at the time under study (middle and late nineteenth century) a growing state sector of the economy. Novak provides a succinct formulation of "public health as a social problem" to which a medical profession in a state of change and uncertainty could offer itself as "the solution":

like any professional body or trade union, the medical profession had to provide sufficient employment for its members. ... entrance into the civil service [through the post of Medical Officer of Health] through the public health bureaucracy meant much more to doctors than an opportunity to improve England's health ... For the doctors, the Medical Officership was a state-subsidized research grant which symbolized the state's need for medical expertise.

Here Novak provides a theoretical interpretation of the relationship between sections of a profession and departments of state which can be applied directly to present day policy processes in health and welfare. His intention was to criticise Macdonagh's acceptance of "government growth" as an INEVITABLE outcome (a "natural history") of increasing knowledge. On the contrary,

Sanitary science was still in a rudimentary state. Hence, when doctors monopolized public health, it was not due to ... their deeper comprehension of disease; it was due to their success in 'selling' medicine to the government. ... this differentiation of knowledge and function was not the natural result of scientific advances but rather an artificial means of ensuring the continued recognition of doctors in the civil service.

As Novak sees it, "professional zeal" is "a dynamic element in the course of bureaucratic growth" AND in the "growth of knowledge". He does, however, cling to a concept of "real" knowledge as something which is in some way "other than" the sort of knowledge claims put forward by entrepreneurial professional groups. This opens him (as it does sociological analysts using a social-problem perspective) to the charge of "ontological gerrymandering"



(Woolgar and Pawluch 1985a and b)<sup>2</sup>. In order to overcome this problem, we will need to follow the development of a more consistent theoretical approach to science as a socially organised activity, in a later section of this chapter. But the contribution of this work by social historians on the relationship between professions and the state forms an essential link in the understanding of present-day social-problem processes and the way in which they result in both "policy" and "knowledge".

Reader (1966) adds to this general account an observation more specific to the case-study taken by Macdonagh (1961), that of the Passenger Acts. The first "inspectors" of sanitary conditions on passenger ships were "naval officers on half pay". Reader points out that at the end of the Napoleonic Wars, there was a large number of servicemen semi-employed (see Reader 1966, chapter 5). Some of these now found a new market for their energies and skills in the inspectorial role under the Passenger Acts, which they fought with great vigour and some sacrifice to improve and implement. A major contribution of these social historians to developing a social-problems perspective is that they remind us to investigate the ways in which social and economic change can affect the circumstances of occupational groups. Such change may give rise to a dynamic (one example of which is the "occupational survival" discussed in chapter 3. below) in which groups must seek redefined roles for some or all of their members. As a writer in the tradition of the "strong programme" of SSK was later to express it:

...professional vested interests may form the middle link which connects, on the one hand, controversies about the nature of phenomena and, on the other, conflict over the availability of resources or the securing of credibility for scientist's work (Shapin 1982b).

This dynamic can, in turn, have far-reaching influence upon what come to be seen as significant social problems at a specific period.

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<sup>2</sup> Briefly, this phrase refers to the practice of writing accounts of phenomena as "socially constructed" only when the author basically does not believe in them, rather than allowing for the socially organised character of perception and understanding in both those cases which we do and do not currently regard as "real".

## 1.4 From “interests” to “Laboratory Studies”

Manning has included some discussion of science as the socially organised production of knowledge claims in his model of the social problem process. In this discussion he refers to the “strong programme” in the sociology of scientific knowledge (SSK)<sup>3</sup>, which regards ALL forms of scientific knowledge-claims, not just the ones which contemporaries regard as “mistaken”, to require explanation in terms of the social activities which produce them. However, he does not deal at any great length with this perspective or the contribution it might make to the study of social problem processes. As the present study lays more emphasis on the “academic” than on the “public” or “policy” debates concerning the health of the unemployed, I will spend rather more time than Manning on the description of recent developments in SSK, and on an attempt to outline a way of combining the two perspectives in a method which may be applicable to the analysis of other policy-related academic controversies. Work in the “strong programme” of SSK has shown that closure of a scientific debate can never be regarded as the result of discoveries about a world “out there”. Specifically, the Duhem–Quine thesis demonstrates that there is no such thing as a universally acceptable “replication” of any scientific experiment (Collins 1983, 1985). Therefore, closure of debate, and the outcome of closure known as “fact” must be studied as social processes. An important contribution to understanding these social processes is provided by the “interest model” developed by (amongst others) Barnes (1977), Bloor (1976) and Mackenzie (1981a).

At the outset of the present study, a guide to theory and method when considering the role played by the researchers (to whom I will also refer as “scientists”) was taken from Mackenzie’s (1981a) account of the development of statistics in Britain in the early twentieth century. There were obvious reasons for this choice: Mackenzie dealt with an occupational group similar to some of the scientists in the unemployment and health issue community; a similar topic: the application of statistical methods to a politically controversial subject; and used a theoretical perspective which I thought would offer a

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<sup>3</sup> An explanation of the principles and methods of the version of the “strong programme” used here is given in chapter 2.

fruitful way of organising some of my material. Mackenzie was concerned with the ways in which the "interests" derived from the social origins and class membership of the participants influenced not only the answers they gave to specific questions, but also the very methods which they used to formulate these answers, down to fine points of mathematical detail. As a result, one of the guiding ideas with which I began the present study was an expectation that the social position and political allegiances of participants in the unemployment and health debate -- in this case not only statisticians but also doctors, economists, and other kinds of academic and professional and paraprofessional workers -- would be found to influence the stances which they took and the ways in which they went about investigating the question "does unemployment cause ill-health?". It seemed that there should have turned out to be a group of participants drawn from non-elite social groups and owing their allegiance to liberal or progressive political currents who took up, defended, and technically elaborated one position ("it does"), and another group, with elite social position or origins and conservative political allegiance, who took up and defended the opposite view.

Mackenzie's thesis to the effect that the development of a new discipline should be regarded as similar to that of a political party (p. 94-95), and that scientific developments must be understood in the context of the occupational roles open to scientific workers (see his analysis of the career of Major Greenwood, p. 110-112) held up very strongly when used as a guide to understanding the relationship between research and policy in the unemployment debate. However, perhaps because of the different historical periods at which the two debates took place, from the beginning of the present study there were difficulties in the use of the concept of "social interests". Paramount amongst these was that the social origins and broad political allegiances of ALL participants were so very similar. There was, therefore, a need to search more widely for a theoretical insight which would encompass the complexity which was beginning to emerge from interview material. To give one example of these problems, the first few "scientists" I interviewed seemed to have great difficulty in remembering what they had actually said in papers and talks about unemployment and health. Interviewed approximately eighteen months after the major Scottish conference on the subject, speakers seemed vague, almost uninterested, and told me that I must understand they had "moved on to other things". This topic, of such consuming interest to me at

the time, was, to the first scientists I interviewed, clearly some kind of a sideline. It obviously would not do to assume that they had carried out some work in this area either out of intellectual fascination or for (covert or explicit) "political" reasons.

This is not to suggest that the political CONTEXT had no influence upon the progress and/or the outcome of the debate as a whole. It is altogether possible, and consistent with the analysis offered here, that there could have been a different outcome if either a social democratic or "liberal" conservative administration had come to power at some time during 1979–87. In section 7.3 it is suggested that a temporary shift in opinion within the political party in power may have influenced the reception of one study. However, as there is no case study such as Mackenzie's with which to compare the present one in respect of political context, as opposed to the social and political origins and allegiances of participants, it must be left to future researchers to explore in detail the influence of political context on policy-related scientific controversy.

One way of addressing the questions which arose from the attempt to apply a concept of "social interests" is to outline the difficulties which other researchers in the sociology of scientific knowledge have encountered. There have now been many case studies using some variation of an "interest model" to investigate scientific debates, and the empirical study of scientists at work (whether or not they were engaged in "controversy") has led to a rich elaboration of this model which increases its usefulness to the study of policy-related scientific controversies.

Critics of the "interest" model developed by Barnes and Bloor and applied by their Edinburgh colleague Donald Mackenzie to the development of medical statistics have asked what would actually COUNT as an instance of "interests determining knowledge" (see for example Woolgar 1981). Woolgar accuses interest-theorists of a

general strategy .. to reveal interests as a kind of backcloth of attendant circumstances, and to imply that this revelation throws into better perspective the knowledge claims or event which is at issue.

In these studies, "interests" are not seen as themselves socially constructed, or as resources actively used by actors. At the same time, they

are seen as "unproblematically available to the sociologist". Woolgar criticizes these assumptions, and, indeed, these were some of the problems with which my data faced me in the early and middle stages of my fieldwork -- I was precisely UNABLE to understand what the "interests" of participants whom I had interviewed were.

Pursuing the problem of how to make sense of the things "my" scientists said in their interviews, and of my own experience as a member of a scientific team, I turned from macro-level studies of science and social interests to micro-level studies of scientists at work in the "laboratory" situation (the term "laboratory" will be used here, as it is in the work quoted, to mean any working situation in which scientists are found, whether or not it contains the accoutrements we would normally think of). Foremost among these both in its relevance to my material and perhaps also in the general estimation, is Latour and Woolgar's (1979, 1986) "Laboratory Life".

In their close observation of scientists at work, Latour and Woolgar developed the concept of the "cycle of credibility". They give this name to what might be regarded as the typical or "normal" career-pattern of a scientist, whereby he or she "invests" his or her time and skills according to a calculation of the best achievable outcome. Such calculation is regarded by scientists as a necessary part of the exercise of their profession, and success in certain individuals is attributed to an inspired ability to make correct investment decisions. The outcome of these decisions, in the form of published papers, invitations to address conferences, and other signs of approval and acceptance, in turn attract resources such as new grants, valued colleagues, additional equipment. This enables the scientist to increase her NEXT investment, with a correspondingly expected increase in returns, and so on.

The value of the concept of the cycle of credibility emerged as my fieldwork progressed. It became clear that, in a policy-related scientific debate, (or at least in the one presently being considered), it can be used to understand the activities, not only of the researchers, but also of government officials, journalists, and members of other groups involved in the issue community (including the voluntary sector's "professional" segment, and those working for political parties) Latour and Woolgar's account not only made sense of scientists' remarks about "time", "moving on", and "getting something out of ..." projects, and of the way in which they lost interest in unpromising areas,

but also of the images projected to me by people in (seemingly) very different occupations who spoke of "having a bright idea and fighting for it", "knowing when to move on", or "getting mileage out of ..." an issue.

One of the most important insights offered by the application of Latour and Woolgar's analysis to the material provided by my interviews with and observations of researchers concerned the use of the concept of "time" by scientific workers. Clarification of the importance of "time" as an investment by researchers is a valuable contribution of the "laboratory studies" to the investigation of the relationship between science and policy making. The allocation of "time" was a concern frequently expressed in my interviews. Concern about the investment of time was not confined to scientists, but was most emphasised by this group, and was the feature which distinguished scientists' accounts of their participation in the debate<sup>4</sup>. The different orientations to "time" and "moving on" of a young academic economist and a civil servant was expressed thus in one interview:

Well, you see, {x'} is a civil servant, an economic adviser. So perhaps he goes on longer on the same subjects, because of their policy aspects. But for people like me [an academic], you take up a topic and then you drop it, either other people take it up or it fades away.

There seemed to be conflicting pressures at work. The academic researcher's time is a part of her means of production, in the Marxist sense: the scientist is unlike some other workers in that she is not totally "alienated" from the means of producing knowledge, because time is a resource over which the scientific worker may retain some degree of "ownership" and control. So, on the one hand, there is pressure for the scientist to ensure that her "time" is as fully "invested" as possible (hence the negotiation of "busy-ness" so commonplace in academic life.) To give a concrete example from my own field observations, statisticians working in medical settings set great store by being "very busy". Of course, they are. But also, it was a signalling of disciplinary and professional autonomy to make doctors, especially, wait in a queue for the statistician's attention. The other side of this social negotiation of "busy-ness" (i.e. the full investment of "time") is, however, that some

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<sup>4</sup> In contrast, "intellectual fascination" and being "hooked" was expressed by non-academic participants, see sections 3.3 and 8.1.4.

(especially less experienced) scientists exhibited, in the present study, a tendency to over-commit themselves at certain stages of their careers. The investment of "time" must not only be made but must be SEEN to be made. This gives rise to a tight and delicately balanced agenda of work priorities for the scientist, in which individual items are highly susceptible to being rescheduled to the bottom of a lengthy list, regardless of the personal interest or commitment of the scientist. In other words, the micro-level negotiation of "time" may, at certain points, leave little space for considerations of "interests", whether these be overtly political or more personal commitments, perhaps derived from the scientist's social and cultural background. To be abandoned by scientists, therefore, it is not necessary that some area of investigation be "suppressed" or "censored". Considerations of time management are a powerful source of control over the setting of the scientific agenda. The successful scientist will know when to terminate a poor investment, when to "move on to other things". The junior scientist learns this skill as part of her socialisation. Although decisions taken according to these criteria may be, at certain points, experienced as a form of "censorship", they will normally be justifiable in terms of setting sensible work priorities, for the good of the laboratory and that of the novice's own career.

In interviews, academic scientists who had made this kind of decision in relation to "unemployment and health" often had notable difficulties in remembering their own previous work. However, far from being a "problem" for them (although it sometimes caused embarrassment in the interview situation), this ability to "move on" decisively was regarded as a product of experience in the proper doing of science in the real world. Knorr-Cetina (1982) has commented on the phenomenon:

Scientists talk about their 'investments' in an area of research, or an experiment. They are aware of the 'risks', 'costs', and 'returns' connected with their efforts, and talk of 'selling' their results to particular journals or foundations. (Knorr-Cetina 1982)

I would like to argue here that Latour and Woolgar's account of the cycle of credibility and the resulting concept of academic "time" as an investment which the experienced scientist learns to deploy artfully allows us to perceive one of the major, if latent and unexplicated, methods of "agenda-setting" within science itself. What this model of scientific decision-making means is that we often have no need to look for more obvious external influences to account for

the appearance or disappearance of certain questions as “scientifically interesting” and “worthwhile”. Because the skilful scientist will pick up far more subtle signals about “what is hot and what is out”, and follow their indications, as part of the normally competent pursuit of an academic career. This is emphatically NOT to say that politically controversial topics are written-out of science, but “in a subtle way”. On the contrary, a topic may prove a good investment by being at a point of balance within a controversy, or may be MADE INTO a good investment by being strategically linked to an “external controversy”. The entrepreneurialism involved here is also a part of successful science. One statistician peripherally involved in the unemployment and health debate, for example, was reputed to have deliberately chosen to devote (untypically for him) some “time” to an environmental controversy well outside his area of greatest expertise because it was “the point in his career” to tackle a “political” subject, which step was considered a necessary one in the process of acquiring a Chair. What is significant here is not whether this story is a true account of the statisticians motives, but that it was repeated as an example by his juniors.

## **1.5 From Laboratory Studies to the “trans-scientific field”**

However, allocation of “time” takes place as between activities which are by no means all defined by the scientific community itself. This analysis of the ways in which individual academic scientists’ and research groups’ agendas are formed points us even more firmly in the direction of the wider social-problem processes which constitute the trans-scientific field. One author who has been concerned to re-introduce consideration of these processes into the conduct of “laboratory studies” is Karin Knorr-Cetina.

In her essay “Scientific communities or transepistemic arenas of research? A critique of quasi-economic models of science” she demurs from the “quasi-economic” concept of investment and return on the grounds that when limited to the interior of “science”, these concepts have insufficient explanatory power. The decisions made in the laboratory

appear to be simultaneously situated in a field of social relations  
(Knorr-Cetina 1982 p. 102)

This is the “trans-scientific field”, which is “organised in terms of



resource–relationships ... the transepistemic connections of research are BUILT INTO scientific inquiry". Knorr–Cetina criticizes Latour and Woolgar for not taking account of the relations between scientific producers and their clients in the creation of "true accounts" of phenomena. She warns against equating the sort of conditionally accepted "information" (knowledge claims) which scientists strive to produce in order to gain recognition by their peers WITHIN the scientific community with a wider concept of "truth". In order for a "truth" to be established, a further step is required, and this she sees as the intervention of ENTREPRENEURS. And it is not only the final step in the fact-creating process (from "information" to "truth") which requires entrepreneurship. Knorr–Cetina also reminds us that science is

a system in which a scientist's ability to work, including the ability to raise money, may depend on decisions made at the top organisational and other administrative levels (p. 112)

And at THESE levels, the scientific cycle of credibility plays no part in the making of decisions, she argues. At this level, questions of allocating resources are paramount. The present study will not exactly follow Knorr–Cetina here, in that what seems to happen at the level at which resource decisions are taken, as far as this can be ascertained, is that certain individuals or subgroups MAY take up a research team's case and push it as a part of their OWN cycle of credibility. The impression given by Knorr–Cetina that somehow there are "big decisions" about resources taken in a different manner from those about "facts" is at least partly an artefact of difficulties of research access to decision-making sites. It is important that this practical question of fieldwork method and strategy not be allowed to construct a monolith at the level of "the state".

Knorr–Cetina does warn of the dangers of merely ASSUMING the relationship between "credibility" and resources. Should we take it for granted that a scientist who is accorded much credibility (through the reception of his or her published work as "true", "excellent" etc) by peers will be more likely to receive further resources to invest in the next phase of the cycle? Knorr–Cetina thinks not, and this forms another of her criticisms of Latour and Woolgar's concentration on the inside of the laboratory. Other laboratory studies have shown the scientists deemed "successful" in this sense have been deprived of their funds and equipment in what seems to them a completely arbitrary manner, or, conversely, find that previously rejected proposals suddenly find

favour in the eyes of those who control access to resources. Although scientists retain partial control of their own time, they are nevertheless still primarily WORKERS in

a market of positions where the commodity is scientists  
(Knorr-Cetina 1982, p. 112)

who must "sell their labour power" like any other worker. Here we can see that Knorr-Cetina is not really objecting to an "economic model" of SCIENCE, but to an inconsistent or incomplete model which fails to distinguish the role of "worker" from that of "entrepreneur" and "employer". An individual scientist may, at various times, behave as if s/he were any one of these (giving rise to such descriptions of academic activity as "time-serving", "getting on a bandwagon", and "empire-building", respectively). But Knorr-Cetina aims at greater conceptual clarity when analyzing the process of scientific investigation, and holds that such clarity will take us outside the laboratory.

Indeed, the concept of a community of knowledge-producers as one composed solely or even mainly of SCIENTISTS is greatly oversimplified. Although I have made use of a concept of "the core group" (Collins 1981) of those who remained longest involved in the centre of the unemployment and health debate, this group included non-researchers. Nor is this concept supported by a naturalistic ethnographic description of what scientists do (Pinch and Bijker 1984). In Knorr-Cetina's fieldwork, as in the present study, the scientists THEMSELVES do not orient solely or even mainly towards each other, depending on the stage at which the process of debate is observed. Much of Knorr-Cetina's criticism of laboratory studies implies the importance of a missing TEMPORAL dimension, and it was the longitudinal character of the present study which demanded the addition of a social-problem perspective to concepts drawn purely from the laboratory micro-studies, useful as these were in understanding much interview material.

In the course of close ethnographic observation:

We see scientists writing letters and sending out papers and grant proposals. We hear them talking to people all over the country on the phone ... We read the correspondence filed away in a folder and learn about contracts realised for industry ... we learn that they FRAME their scientific work in terms of their ex situ involvements (Knorr-Cetina 1982, p. 117)

Furthermore, these negotiations are woven tightly into what will ultimately

result as a knowledge claim. The network of relationships in which the scientist is involved thus enters fully into the resulting "facts". Knorr-Cetina does not regard this network as "directly observable". In the present case study, an attempt has been made to observe a large part of such a network, tracing those connections perceived as significant by scientific (and other) participants by means of reading the documents and correspondence produced and referred to, and by interviewing members of the groups making up the "trans-scientific" field for the academic debate.

We are left, still, with the problem of finding an alternative to the mere assumption that "credibility" is automatically translated into resources. When scientists reach out along their networks seeking for resources, what do they offer in return, what are they "selling"? Knorr-Cetina suggests that it is

the convertibility of ... a scientist's work ... into the locally relevant "currencies" (p. 121)

That is, we cannot even assume anything about what may COUNT AS a resource, either sought for or offered in return by the scientists in their wider relationships. It MAY be money or equipment or space or time. It may be legitimation. The fragility of the definition of a "resource" is reflected in the amount of effort put into writing research proposals, she argues. Furthermore, and here again the social-problems perspective is a good guide, scientists may seek to convert what they already have (a new statistical measure, for example, or a medical procedure, or a "unique" set of data) into a resource relevant to some other group's own aims. What this means is that the analyst cannot be too quick to make simple assumptions about what will be sought for or offered as a resource. Rather, it is necessary to listen to what subjects say. As it was observed in the unemployment and health debate, so also in Knorr-Cetina's study

Oscillations between conflict and co-operation, between the fission and fusion of interests that are reciprocally defined, are routine correlates of the process of negotiation which characterizes resource-relationships (1982, p.122)

Accordingly, the analyst must not assume that somehow "society" provides an input into science in the form of setting social problems for science to solve (see section 3.1). Scientists are not passive recipients of either resources or "questions", but rather active participants in the social-problem process through which "problems" are defined. Social and political issues may reach

deeply into the design and analysis of research. But equally, available and/or well-marketed scientific skills and techniques will influence what comes to be solidified as a "researchable issue". Furthermore, scientists (and policy makers, and research funders and managers) must always be to some extent engaged in a process of striving for or approximating a desired relationship between concerns deriving from the trans-scientific field (the social, medical, etc. "problems" at stake) and the design of measures, tests, samples, and other "laboratory" procedures. Even when there appears to be complete agreement between customers and contractors:

Scientists "figure", "gather", "think" and "hope" that a particular problem translation ... will match the interest of those to whom they are committed, but they often do not KNOW EXACTLY what is expected from them. So they re-direct their guesswork according to the responses they get, and they may end up convincing those who are "interested" in the work about what exactly they should be interested in. (Knorr-Cetina 1982, p. 124)

Out of this negotiating, guessing and figuring not only about the natural world but also about the characteristics of the research team's SOCIAL world which need to be correctly managed in order to advance the (individual and collective) aims of team members, emerge "facts" and "knowledge".

## **1.6 The "translation" model and the social-problem process**

The most recent and sophisticated work on the process of "translation" between scientists' concerns and those of significant groups in the "trans-scientific" field comes from France (Callon 1986, Callon 1981, Callon Law and Rip 1986, Latour 1984a and b, Coutouzis and Latour 1986, Latour 1987). An account of the theoretical and methodological bases upon which this research is constructed will show the close relationship between a consistent constructivist programme for the sociology of scientific knowledge and the social problems perspective.

Callon and Law (1982) see the "networking" activities of scientists as "an attempt to determine the relative 'marketability' of different fields of work", for example, in choosing what questions to pursue, selecting the journals to which they will send papers, the funding bodies to whom they will appeal, and so on. All these activities involve attempts to first "interest" (this term now being used

with the specific meaning of "actively attract the interest of") and then "enrol" others in the scientists' own projects:

Analytically, [the scientists'] position is little different from a politician who uses argument and persuasion to insist that it is in the 'interests' of this or that social group to vote in this or that way (Callon and Law 1982)

Callon and Law do not reject the concept of "interest". But they use it in a somewhat different way to that of, for example, Barnes and Mackenzie (1981b). Inter-esting, or "interessement" is an ACTIVITY in which scientists engage, for example, when they target a paper at a specific journal or a grant application at a particular funding body (Law 1983). Furthermore, their concept of enrolment is not unidirectional. If a researcher "sells" a paper to a journal, s/he has enrolled the journal in his or her scheme of things. If the editors ask for major changes and get them, however, then it is the SCIENTIST who has been "enrolled". The question of "interests", for Callon and Law, is therefore

to discover how it is that actors enrol one another, and why it is that some succeed whereas others do not.

One strategy of enrolment is to guess what the interests of other ARE, and adapt one's work accordingly. Scientists do this, it is proposed, by constructing "interest maps". That is, they attempt to construct a picture of the temporarily stabilised outcome of previous processes of enrolment which will enable them to steer towards their own goals. Callon and Law distinguish themselves from Barnes and Mackenzie in two ways. The first is that they regard "others' interests" as shifting and temporary rather than as a "backcloth" which can be taken for granted in the analysis of the work of a specific scientist or research team. The second is that they regard the appeal to interests as only ONE way in which enrolment can be attempted. In this, they do seem to approach close to Woolgar's (1981) formulation: that is, that scientists actively engage in explicit "interest-WORK" and that this should be a topic for the sociological analyst. There is a considerable contrast between this position and the way in which Barnes and Mackenzie treat "interests" as properties of social GROUPS, which individual members may not consciously hold. (Mackenzie 1981b). For Callon and Law, interpreting other actors' "interests" as indicators of THEIR location in a social structure is an activity by which members "do structure-work", that is, construct the social world as a stable enough place in which to plan and pursue goals.

On this view, interests ... are attempts to define (and most importantly, to enforce) the institutions, groups, or organisations that exist from time to time in the social world (Callon and Law 1982)

The debate on the status of "interests" has moved from a position where some critics of the Edinburgh school have feared that scientists were being constructed as "cultural dopes", through to a position where everyone is artfully negotiating their own and each others' concepts of what is "really in their interest". Lynch, for example, condemns studies of scientists which treat them

as if [they] were deprived of adequate access to their own socio-historical circumstances (Lynch 1982)

He argues that scientific and technical workers have all sorts of ideas about the outside world, and consciously decide what about this world is relevant to their present work and how to deal with it. He calls this "critical inquiry", and recommends the sociological analyst of science to study it. Such study must be done ethnographically, at close range, and preferably following a debate in "hot blood", (see section 2.2) because publicly, scientists deploy a rhetoric which "hides its drama in the form of a response to necessity", that is, scientists construct their public talk as an inevitable response to a solid world of facts "out there" (Latour 1981). Inside the laboratory or the office, researchers are constantly dealing with this "drama" -- the "troublesome questions" about the relationship between the outcome of technical exercises (experiments, computer runs) and the real world. Factual claims by other competing researchers are translated back into social artefacts or psychological aberrations. Scientists, in other words, Lynch shows, are apt to construct EACH OTHER as cultural dopes, as a part of the very process of competing claims to "truth". All these types of reasoning and rhetoric were clearly visible in the ways participants (not only scientists) spoke of each other's work in the unemployment and health debate.

This working-through of the consequences of regarding science as a thoroughlygoingly social activity leads back towards the social-problems model described in section 1.2., and to the ways in which an integration of the two approaches can enable steps forward to be taken in the understanding of policy-related scientific debates. A "SSK" which locates scientists so firmly as active subjects within a social world in which they participate as

knowledgeable, artful and striving to attain certain personal and group objectives seems to fit well with a programme to treat social problems as organised phenomena with their own variable "social histories". As Latour (1981, 1987) insists, there is nothing sacred or special about "science. It is a social activity amongst others. So it should not come as a surprise that concepts found useful in the analysis of scientists' activities, in the present study, proved applicable, with some modification, to understanding the activities of members of other groups in the "issue community". Latour (1987, p. 168) advocates that in studying the fate of scientific ideas

we should not consider only those who called themselves scientists -- the tip of the iceberg -- but those who, although they stay outside, are nevertheless shaping the science, and form the bulk of the iceberg.

This is necessary because

scientists ... link their fate to that of other and much more powerful groups ... that have learned how to interest everyone in some issues ... groups that are constantly on the look-out for new unsuspected allies ... (p. 169)

The scientists entered the social-problem process in the same way as other groups, indeed, the conduct of the academic debate only began to become comprehensible when scientists' activities WERE theoretically approached in this way.

The strong programme, by opening up all forms of scientific and technical activity to sociological analysis, allows us to see that scientists are like any other participants in organised activity who compete for material and cultural resources. They make claims in relation to other scientists, and also in relation to the wider society. Just as social problems arise from the ongoing and related activities of social movements, professions, officials and journalists, so do "scientific facts" arise from the ongoing activity of scientists IN RELATION to these other groups. At certain times, scientists of one sort or another are "mobilised" by other actors in social-problem processes, to produce factual claims consistent with these actors' value orientations (see Harwood 1979, 1980, 1982; Gillespie, Eva and Johnston 1979; Nowotny and Hirsch 1980). At other times, scientists themselves will enter a social-problem process, or even create one, in order to advance the claims to status and resources of the research team, laboratory, or other group or institution to which they belong (as in Aronson 1982a and b). Scientists enter the fields of social problem

debate “spontaneously” when a group of them can see that the debate offers opportunities for new and desirable alliances, that is, for “mobilising” others in pursuit of the scientists’ own objectives. If these objectives can be attained by alternative means, then participation in any given debate or social–problem process will come to be seen as of only passing relevance. In the present case study, one major feature of the social history of the issue is the way in which various researchers made sallies into the field (“unemployment and health”) and then WITHDREW, a course of action which they themselves regarded as typical of “scientific work”. Another feature, however, was that OTHER groups (activists, officials, journalists), treated the issue similarly: as a means to their various ends, so that at any one time, groups could find themselves left “high and dry”, without allies to support either their moral or their factual claims.

In Latour’s formulation, chains of reasoning which scientists construct forge the links of social alliances. Logic and social strategy are one and the same. A “fact” will be established when one group establishes their argument (or artefact) as a common element which links together (perhaps provisionally and fleetingly) the majority of the groups involved in a given social–problem process. This he terms an “obligatory point of passage”. For example, the success of germ theory in nineteenth century France was that its protagonists eventually established the “microbe” as the common element to all the varied mass of phenomena (both “biological” and “political”) with which a far greater and older social movement (“Public Health”) was concerned, thus mobilising a strong ally (Latour 1984a, p. 25–35).

From this account of the translation model of scientific controversy, we can clearly see its close similarities to a social–problems model of policy debate. In effect, Latour, Callon and their colleagues have demonstrated that the construction of fact and artefact and debates over questions of “policy” are inextricable. If the researcher adopts the ethnographic method advocated by the “laboratory studies”, and persistently follows or “shadows” (in Latour’s terms) the scientists in their daily work, it can be seen that an essential part of the activity of senior scientists entails discussions and negotiations with a wide variety of decision–makers outside of the “scientific community”. Similarly, in the study of policy debate, activists and officials will be found to engage routinely in activities to “enrol” support on the basis of factual claims. Latour and Woolgar (1986) quote Gusfield’s work on auto accident prevention policies in the USA as “exemplary” of their own approach. Other examples of work



which has started from the "policy" end and converged towards the conclusions of those who begin by studying "science" include Petersen and Markle and their colleagues on cancer therapies (see Petersen and Markle 1981, Markle and Petersen 1981, McCrea and Markle 1984), Johnson and Hufbauer (1982) on sudden infant death syndrome, and Troyer and Markle (1984) on coffee drinking as a social problem.

A recent example of an approach combining insights from social-problems theory and the sociology of scientific knowledge is Aronson's account of nutrition policy in early twentieth century United States. Aronson (1982a) shows how a long drawn-out effort by Atwater, the leading entrepreneurial figure in the new subdiscipline of nutrition, produced "dietary ignorance" as an obligatory point of passage which mediated the concerns of a new science, the US government, and American labour reformers who wished to improve the lot of the industrial worker and quell civil unrest without changing basic economic structures. Labour movement agitation around the inadequacy of wages provided the occasion for the scientists to raise the problem of nutritional sufficiency onto the political agenda. Atwater and his colleagues then technically and morally fragmented the question into a problem of measurement (technical) and one of education (moral), and in so doing established their knowledge claims as important "facts" for use in the policy making process.

An even more recent example is provided by the work of Christine Crowe on the in-vitro fertilisation debate (Crowe 1987). Crowe gives an account of the construction of the concept of the "pre-embryo" as the obligatory point of passage which mediated the concerns of the subprofessions of obstetrics and gynaecology, the State, and pressure groups representing conflicting attitudes towards women's rights and abortion. The fragility of these alliances, and therefore of the "reality" of the pre-embryo as a biological entity, is clearly seen in this sensitive account which documents research done "in hot blood" during the course of an unresolved debate.

To complete the argument of this chapter, I would like to suggest that there is a reading of the work of the French "translation" school which can to some extent "domesticate" their own propositions. Rather than regarding the recommendation to "shadow the scientists everywhere" (and, if my argument is accepted, the activists, the officials, etc) as open ended and therefore

methodologically very demanding, it may be that we can see a pattern in the participation of scientists, and other groups, in social problem processes. In all of the case studies discussed by Latour, Callon and their colleagues as well as the other English and American studies cited here, it seems that the important groups were of three types: a scientific subdiscipline, a subprofession, and one or more interest-groups within the administrative agencies ("the State")<sup>5</sup>. Activity in pressure-groups and "intermediate bodies" outside the "State", including the provision of "information subsidy" to the media acted as the vectors between these<sup>6</sup>. This is very close to what is proposed by Manning as the procedure for investigating social-problem processes. It is the model which will be used in the present case study.

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<sup>5</sup> see, for example, the discussion of military, public-health, and general physicians in Latour (1984a, chapter 3), also Latour (1987) chapter 4 and pages 255-256.

<sup>6</sup>In other types of social-problem processes, perhaps the majority, groups engaged in private industry will constitute a fourth party, for example, brewers and distillers, auto manufacturers, coffee or pharmaceutical manufacturers, etc.

## CHAPTER 2

### METHODOLOGICAL EXPLORATIONS

Chapter 1 has outlined the different theoretical perspectives which have been adopted and combined in this study, and described the ways in which “gaps” and problems arising from the use of each one separately led to a search for elaborations and refinements which would allow field data to be more satisfactorily organised and understood.

This chapter has 3 purposes:

1. to specify in more details the “principles” and “rules of method” which have been adopted from the “translation” approach to the sociology of scientific knowledge
2. to describe and discuss the types of fieldwork undertaken in this study, and describe some of the problems which arose during the collection of material
3. to consider the problems involved in making general statements, whether these purport to be “explanatory” OR “descriptive, on the basis of a single case study

Workers in the social-problems perspective and in the “strong programme” of the sociology of scientific knowledge have not been greatly concerned with the problems of method which arise during fieldwork and documentary research. Some time must therefore be devoted here to a consideration of the methodological issues involved in any case study, and in doing the sort of interpretative work demanded by sociology, social history and ethnography. Although a social-historical account of a policy-related academic debate has been most helpfully provided by Aronson’s (1982a and b) work on nutrition science in the early 20th century, neither she nor others working in the “social problems perspective” seem to have given a great deal of attention to discussion of the problems involved in assessing the validity of accounts.

Hugh Heclo has constructively criticised some of the assumptions which have underpinned past case study work. The case study is intended to illustrate wider points, Heclo reminds us, and, ideally, should be (as any other scientific enterprise) “Planned under the impetus of theory” (Heclo 1972, p. 88). This he calls the “deductive” use. The “inductive” use of the case study is to build up a number of cases, as done in clinical medicine, which eventually elucidate the course of a disease. Heclo feels that behind the common inductive use of case

studies, lies the idea that at some point a "critical mass" will be arrived at, which will somehow of itself produce a theoretical breakthrough. Unfortunately, at the time of writing, he saw little sign of this object being achieved. We should, not, however, abandon the method:

There appears to be nothing about the case study technique which is inherently nontheoretical or unscientific: the problem lies in assuming that theoretical contributions will emerge automatically from narrative. (p.93)

The advantages of the method are that case studies provide a way of emphasising changes through time, and of concentrating on dynamic factors, on "how things work" (p.93). The fact that case studies can make use of a variety of fieldwork methods is also seen as a strength. However, Heclo demands that every researcher presenting a case study be required immediately to answer the question "Case of what?". At this point, the methodological problems of policy studies elide with those of the sociology and social history of science. (Shapin 1980 p. 134, D Smith 1982, Collins 1983) I will begin to address these problems by describing the "rules of method" used in the studies of scientific and technological knowledge which have guided those aspects of the present study concerned with understanding the actions of the researchers involved in the unemployment and health debate.

## **2.1 "Rules of method" in social studies of science**

Because the ideas involved in the present work are contemporary, it has been possible to investigate their development using an ethnographic method similar to that used in the "laboratory studies" (Latour and Woolgar 1979, Knorr-Cetina 1981, 1982, Gilbert and Mulkay 1984), extended into an attempt similarly to observe parts of the "transepistemic field". It is more difficult to find examples of observational studies of other groups involved in policy-related academic debates such as policy-makers, and funding bodies, but we might take Heclo and Wildavsky (1972) on British higher civil servants as one example, and Kogan and Henkel (1983) on the functioning of the Rothschild research structure within the Department of Health and Social Security as another. In such an approach, a case study may be seen as an attempt to provide both a social-historical and an ethnographic account of a

policy debate. As has been argued in Chapter I, this is the direction in which recent work appears to lead.

Latour and colleagues (Latour 1984a, 1987, Coutouzis and Latour 1986), Callon (1986), and Law (1986b) have provided a series of methodological rules and principles for the conduct of ethnographic studies of scientific work. This is referred to as the "translation model". For the present purposes, "scientific" work will not be differentiated from other kinds (professional, communicative, didactic, etc) carried out by members of groups in a policy community, and therefore the "translation" perspective will be regarded as applicable outside of what is conventionally defined as "science". So the study of the work of officials, activists, and journalists will also be approached using the same methods.

The first "principle" of the translation model is that the analyst should avoid "drawing up a double entry ledger" (Latour 1984a) with science on one side and society on the other. Instead, the investigator should "follow the course of the action wherever it leads" (Coutouzis and Latour 1986). The first principle thus recommends a similar procedure to Spector and Kitsuse's method for the study of social-problem processes. And what has been discovered by the use of this method is a process of "stages", in scientific and technological debates<sup>7</sup>. They add that scientists and technologists themselves do not divide society from science, but artfully negotiate the stages of claims-making process in which they are engaged (Latour 1981).

The second principle is to "begin with controversy" (Coutouzis and Latour 1986), that is

cases where the experts themselves disagree and are themselves  
(to the advantage of the observer) engaged in discussion of each  
others positions, presuppositions and prejudices

If one starts from controversy and makes no presuppositions about what is fact, or even who is expert, it is found that a discussion will often "mix experts,

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<sup>7</sup> Latour, Callon and their collaborators extend their perspective to embrace the study of technology, by regarding the acceptance of new ARTEFACTS as the outcome of strategic social action in the same way as the establishment of new "facts", see Bijker, Hughes and Pinch 1987, p 83-106.

politicians, and the rank and file", all of whose roles must be equally attended to if the process of a debate is to be understood (Coutouzis and Latour 1986)

The third principle is that of "maintaining symmetry" (as in Bloor 1973, 1976, Mulkey and Gilbert 1982). This is an important precaution given the necessity to study controversies, as the observer risks becoming "victim of the actors' own partiality". We must seek to explain both "successful" and "unsuccessful" claims ("true" and "false" hypotheses) in the SAME TERMS. There can be no appeal to unexplicated notions of "rationality" or "the outside world" to explain the success of some theories only. Nor should consideration of social or political factors be absent from the explanation of "truth" and confined to the explanation of "error". Those claims which become accepted as fact, as well as those which do not, are regarded as produced by political, economic, social, psychological and material factors, without privileging one or the other. It should be emphasised here that in the present study the "outside" world, or "nature" is not taken to be either absent or dominant in the explanation of scientific controversy. Rather, the material world is regarded by the sociologist (as by scientists themselves) as a potential ally or opponent in claims-making strategies of the same importance as institutions, businesses, funding bodies, other scientists, etc. Nor is it to preclude observing that scientists (and others) judge and comment on the "quality" of each other's work, judging some to be "good science" and some to be "bad science". The principle of symmetry does demand, however, that the sociological observer be sensitive to the contextual and temporally specific nature of such judgements.

This leads us to the fourth principle of "explanation by association and situation". The sociologist

cannot give an account of victory or defeat except by means of the presence or absence of allies and powers at the right time and in the right place (Coutouzis and Latour 1986)

In the application of these principles to a case-study of the transfer of a technology (solar power) to a developing region, Coutouzis and Latour found that they offered great advantages, foremost amongst which was that

one can follow controversies in hot blood, even the most bitter ones, whether they be about electric cars, gravity waves, neutrinos ... Instead of arriving at the end to distribute prizes to the heroes and brickbats to the villains, this method does not hesitate to enter the situation where there is everything to play

for (Coutouzis and Latour 1986)

This makes the perspective proposed in the "translation model" well suited to the understanding the unemployment and health debate and, I would argue, other cases of policy-relevant scientific and technological controversy.

What emerges is a series of "stages" in a debate. Once groups do enter the process, they engage in:

- 1) attempts to **"interest"** other groups -- whether these be conceptualised as "social" (funding bodies, other professional groups, political parties) or as "natural" (microbes, as in Latour (1984a), shellfish, as in Callon (1986), tides, as in Law (1986).

Here "inter-esting" ("coming between") is taken to mean persuading a group's members to take a different course, to take a diversion down the road which the scientists (or other would-be "inter-estors") propose. It is achieved by a form of bargaining or compromise between the ongoing activities and objectives of the scientists and those of the potential allies. The process of "inter-esting" entails

- 2) **"translation"**, that is, mutual adjustment of the groups' ongoing activities so that they become "resources" for each other (see Knorr-Cetina's discussion of the ways in which items may be constructed as "resource" in section 1.5. above)

If translation is successful, groups have succeeded in

- 3) **"enrolment"** -- the creation of a (relatively) stable set of alliances. Out of this "lash-up" (Latour 1987) of forces emerges an OBLIGATORY POINT OF PASSAGE. That is the statement or instrument which all groups accept as essential to their several purposes, the expression of the point where their "interests" meet.

If the alliances hold, the Obligatory Point of Passage (OPP) will be transformed into an unquestioned BLACK BOX (a phrase first used in the sociology of science by Whitley, 1972), whose solidity and reality is accepted far beyond the boundaries of the alliance which created it (Latour 1987, pp. 138-139). Latour and Callon are at pains to point out, however, that there is nothing inherent in the fact or artefact which makes it solid. Rather, it is the extension of the networks along which the blackbox can be passed without

being "broken apart" which CONSTITUTES its solidity. The example given in Latour and Woolgar (1979) of this process is the referencing of statements in scientific papers. A statement which is a true "blackbox" will not be considered to need any reference at all. One which is considered fairly solid will need, perhaps, one standard or "classic" reference. A highly doubtful statement needs to be sustained by a host of tactically chosen references which attempt to "lash-in" the reader within the network of the statement's producers. This is the point at which the concept of entrepreneurialism emphasised both by social-problem theorists and by Knorr-Cetina (1982) can usefully be integrated into the analysis.

Callon (1986) devotes considerable attention to "dissidence and betrayal". Allies can turn their coats, for a whole series of reasons. Experiments may fail, more powerful interests may beckon, an OPP may simply cease to be so because participants discover other ways to attain their objectives (p. 219-220). Furthermore, a claims-maker may overreach herself -- she may, in the attempt to recruit yet more allies and extend the network of those who accept the blackbox, stir up previously quiescent opponents (Collins 1985, p. 134). A new machine, which works well at sea level or in a developed economy, may fail in other environments and cause its very principles of construction to be questioned and its inventors' statement to be discredited. A fact or theory may begin to be questioned BECAUSE it has attracted new allies who attempt to extend its applicability into jealously guarded new territory. This hazard formed a concern for participants in the unemployment and health debate. To be "seen to be doing something policy-relevant" attracted powerful new allies to the scientists. But at the same time it placed scientists' work under a new and threatening form of scrutiny. In some ways, the concepts of "interesting", "translation" and "enrolment" resemble that of "partisan mutual adjustment" as described by Lindblom (1979). Such mutual adjustment would tend to be required in precisely the kind of "policy communities" which Haywood (1983), Richardson and Jordan (1979) and Ham (1985) describe as the location of decision-making in modern Britain. This similarity in concepts used by sociologists of knowledge and social policy analysts seems to point to an opportunity to develop a more integrated perspective for the examination of policy-related academic debates.

Following the stricture of Heclo, I will specify: "A case of what?" The present study is taken to be a "Policy-related scientific debate", and will be



approached with the aid of the models of social-problem processes developed by Kitsuse and Spector, Downs, and Manning; and approaches to analysing scientific controversy developed within the tradition of the sociology of scientific knowledge. These theoretical perspectives allowed me to understand the role of entrepreneurial subprofessional and subdisciplinary groups in creating unemployment and health as the kind of social problem which required the application of inspectorial, scientific and practical/clinical expertise of various different kinds. It will be argued that a "crisis" in a subprofession created by a combination of administrative and wider social changes (outlined below in chapter 3), was the essential driving force in the social-problem process.

Plummer (1983) writes that "in the case study the theory is there at the outset to weave together the document..." He contrasts this to Glaser and Strauss' (1967) concept of the case HISTORY in which the fullest possible story is obtained for its own sake, in the hope that theory can somehow be "based on" it. The latter concept obviously runs up against the objections stated by Heclo discussed above. The present study is therefore not an attempt at a detailed "history". However, neither were theoretical concepts established at the outset and maintained unchanged to the end. In this respect the procedure here does more closely resemble Glaser and Strauss' methods, in that it involved an "intimate acquaintance" with the world of participants, and also a flexible attitude to "ad hoc conceptualisation" which took place at various stages of the research. A description of this process can best be integrated with an account of the fieldwork methods used.

## **2.2 Fieldwork methods**

A combination of fieldwork methods have been used, very much as envisaged by Heclo (1972) and by Spector and Kitsuse (1977), that is, a combination of participant-observation documentary research and interviewing. I carried out 59 interviews with participants in the debate, over the period May 1983-June 1985. Interviewees included the first authors of all British papers on unemployment and health which gained the attention of the media and of politicians and were considered "central" by participants throughout the debate. A number of civil servants who had had responsibility for this issue were also

interviewed, as were leading "activists" in the pressure groups, including the organisers of the two Scottish conferences. Interviewees were distributed amongst professional activities as follows:

	Number	Number engaged in pressure-group activity
Academics	23	
of which:		
Sociologists	7	5
Statisticians	5	3
Economists	5	1
Community Medicine	3	2
Psychologists	2	not known
Nursing	1	0
Civil Servants	12	3
Workers in "intermediate bodies"*	14	7
Journalists	5	not relevant
Doctors "in practice"	5	
of which		
Psychiatrists	2	1
Community Physicians	2	2
GPs	1	1
Total	59	

\*includes charities, independent research and/or policy units and "quangoes".

Two people declined to be interviewed when asked, one because he felt his work had not been central or important enough, the other was a "key informant" whom I had known very well for some time, and felt that a "formal" interview would be incongruous and embarrassing. However, we had regular conversations throughout the fieldwork period, from which emerged essentially the same type of information as that derived from interviews.

No attempt was made to pre-determine the numbers of people in each "category" who should be interviewed. The definitions of the categories changed over the course of the study, and the above list is based on the respondent's **source of income**, so that, for example, several

intermediate-body workers were sociologists or economists. In fact the difficulty involved in separating "activists" from "academics" or "professionals" was one of the most important stages of the case study. This difficulty led to the observation that professional and disciplinary subgroups could use pressure group activity as a part of their efforts to increase the strength of their claims to "expert" status. Greater emphasis was placed on establishing ongoing relationships with a number of "key informants". For example, a small number of journalists gave me fairly detailed accounts of the processes by which certain items "made the papers" (see section 6.2.2, 8.2.1), and these accounts, rather than a large number of one-off interviews, were used to construct typifications of "information subsidy". It was also fortunate that my own personal and professional history made it possible to establish a "key informant" relationship with most members of the "core group" of researchers who remained at the centre of the debate during the period 1981-1987.

Interviews were semi-structured attempts to elicit participants' own accounts of how they came to be involved in the debate, and how they envisaged its future development. The approach adopted, and many of the problems encountered in "elite interviewing" are discussed in Dexter (1970), and Seldon and Papworth (1983).

The interviews had several aims:

1. to elicit the "discovery stories" of participants
2. to discover how researchers' work came to be taken up by journalists, officials and pressure groups,
3. to discover as much as possible about how decisions as to the policy-relevance of the research were taken

As often happens, those who were interviewed contributed to and changed the very objectives of the study itself. Seldon and Papworth have pointed out that an important outcome of "elite" interviews is that they enable the researcher to begin to fill-in the assumptions that lie behind documentary evidence. Another role played by some interviewees, was to instruct the fieldworker where to look for the important documentary evidence (Seldon and Papworth p 43). Seldon and Papworth quote de Weert and Vorkin (1982), who found that

"An interview is not the end of a topic, but the beginning, a conduit from the black and white world of published papers to the colourful and chaotic real world of science"

It should also be pointed out that, as Payne et. al (1981) warn the field researcher, the interviewing and the participatory phases of the research "got to me", became a "key part" of my life, which "drained" me intellectually and changed me emotionally (Payne, Dingwall, Payne and Carter 1981, p 207–208). Nor was this a one-way process. By the closing stages of writing up this research, I was involved in collaborative work with two of "my subjects", and was obliged to acknowledge the possibility that my "participant" fieldwork method may have fallen short of "leaving the situation untouched"<sup>8</sup>.

The institutional context within which the debate took place was investigated by documentary research on the history and functioning of the various institutions involved (research institutes and funding bodies, professional associations, departments of state, pressure groups). Participants' perceptions of the way the institutions function were gleaned from participant observation and interviews. Much that happened in the debate before my own period of participant observation had also to be gathered from documents and interviews. The ongoing processes at the micro-level in both one of the research departments and one of the pressure groups were investigated by participant observation. Some of this was undertaken before the present project, when I worked as a member of a research team which became involved in unemployment and health. It is therefore undocumented by field notes, and relies on my own growing awareness of the sociological significance of the process of integration which resulted in a form of somewhat marginal "membership" of the research community. Throughout my interviewing of "scientists" involved in the unemployment and health debate, I made notes on the ways in which my own socialisation allowed me to employ shared assumptions as a way of facilitating the interviews. At a later stage, it also became clear that the particular TYPE of membership (junior) I had attained limited my ability to "understand" some of what the scientists were "really talking about". In a rather disturbing process, my increasing sociological understanding of the debate caused me to re-write a great deal of my own previous experience as a member of the scientific community. This analysis of

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<sup>8</sup> However, Latour (1988) has criticised the notion that the social study of science should set itself up as yet another discipline with the same characteristics (need for status, preservation of rigid boundaries, etc) as those it studies. He goes so far as to *recommend* "blurring the distinction between science and the production of other sciences" and to favour co-authorship with scientists who have been the "subjects" of one's work (p. 175).

my own store of members' knowledge constituted the major input from the first period of "participation".

The period of "native participation" in the research team may be thought of as the first phase in the process (it would not be accurate to describe this phase as "fieldwork") which produced the present case study. The second phase was a series of interviews carried out in Scotland with participants in the specifically "Scottish" period of the debate. I looked upon this part of the research as a pilot study, and material from this phase of my study has only been made limited use of. The Scottish interviews did not sufficiently clarify my ideas to allow me to proceed investigating the wider British debate with a polished "model", but established at least the outlines of a theory about the nature of the debate. In the course of the Scottish "pilot", I decided whom to interview on the basis of two criteria. Firstly, all (at least) first-authors of published academic papers were interviewed. Other interviews were decided on the basis of a "reputational method". As well as the published authors, there were people around who knew other people who had been involved in the unemployment and health issue. Just finding out who these were and how they knew each other provided valuable data on the actor-network which created, and was created by, emerging definitions of what the "social" and "scientific" issues were. I decided to begin by unstructured interviews, using a tape recorder, to simply elicit peoples "stories" of how they had become involved. The theoretical working model I was using at that time was two-fold. I thought I would find that scientists' interpretations of the debate would be influenced by their social origins and allegiances as in Mackenzie's work on biometry. My second hypothesis (rather vague) was that pressure groups and the media were influential in the effect of research on policy decisions, and that insofar as pressure group members and journalists were unable to grasp the complexities of scientific reports, their role could well be to "distort the relationship between research and policy"<sup>9</sup>.

No sooner had I carried out my first few interviews than I discovered that people who were kind enough to talk to me often felt that they did so at some

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<sup>9</sup> For a statement of the "distortion" theory of the effect of pressure groups and the media on policy-related scientific debates, see Elwood and Gallagher 1984. The fact that this comes so close to my own early view in itself shows the extent to which I had absorbed the "natural attitude" of one of the customer-disciplines to community medicine (epidemiology).

risk, that much of what they told me was regarded as extremely confidential, and that access to further participants in the debate seemed to depend on ones who had met me "putting in a good word". I also found it easiest to allow myself to be cast in the role of seeker after knowledge in the substantive area (the "trainee" role which many other fieldworkers such as Schlesinger (1980) and Finch (1984) have also found themselves occupying). This appeared to me to rule out the use of tape. For that reason, I relied on a developing ability to write quickly, and to remember afterwards a lot of what had been said which I had not managed to write down during the interview. Of course, this method of recording is open to bias in recall, and being affected by what seems most important to record at the time of the interview. I did try to write down things which seemed to "make no sense" in the terms of my working hypothesis (these were harder to remember afterwards), and to include notes on when the interviewee, for example, had difficulty in remembering certain events. Much of this puzzling material, for example references to "time", and memory lapses (see section 1.4) later came to seem very significant, which may be a criticism of not having used tape. However, I felt at the time, and still feel (perhaps with rather less certainty) that the use of tape would have made some interviews impossible and produced an unhelpfully guarded response in many others.

During the course of the "pilot" it became clear that at a certain point the issue community became "saturated", that is, the last person you interviewed advised you to talk to someone you already had. I termed this "network closure", and it seemed a good way of knowing what constituted the effective issue community and when one could stop looking for more views, opinions and "discovery accounts". Later on I discovered that as one's theoretical understanding of the debate changed, the network opened out again and aroused the horrifying spectre of an endless number of people one ought to interview. These new, theoretically defined networks, however, led far beyond the bounds of the thesis topic, although into interestingly related areas. So that the requirement of "sticking to one's topic" (which was "the unemployment and health debate", and not "the role of economic advisers in government", or "the customer-contractor relationship in health services research", or "the social construction of epidemiological knowledge") provided the final boundaries of the network of people to be interviewed.

One other problem connected with deciding who to interview was balancing the value of the material possibly gained against the degree of suspicion and

embarrassment involved in talking to prominent people about a politically sensitive topic. In particular there were known to be four leading academics who had for a brief period played a role in discussions with civil servants on what research was needed. No-one would tell me who these persons were. I was able to gather a fairly good idea of three identities, but when I approached two of them, they made it clear that they were not willing to talk to me about "the debate". By this time I was finding it increasingly irksome to be put in the "novice" role and lectured on the inadequacies of "my study" as a Ph. D. in community medicine or epidemiology, which was the format taken by several interviews with very senior (and not so senior) "experts". As a result, I did not pursue the "four professors" further, which may also be a criticism of my study. My formal justification for this decision was that none of the "four professors" were published authors on the topic of unemployment and health, as far as I knew.

Interviewing shaded into participant observation at many points in my fieldwork. I met members of the pressure groups (the Unemployment and Health Study Group and the Social Costs of Unemployment Forum) by interviewing them, and continuing to take some part in the group's activities (to an extent limited by distance and certain other considerations) seemed to follow on in a "natural" way. Participation in the pressure group's activities continued for over three years, and resulted in the usual degree of "going native". I was always kept informed of the background to any activity, such as the production and release of the group's major report, the writing of press releases on other research to which the group wished to draw public attention, and so on.

The close association of the pressure group with one subset of the "entrepreneurial sub-profession" resulted in a further involvement with the related journal (Radical Community Medicine), on whose editorial group I spent a period of one year. In the winter of 1984-5 the journal, which, like the pressure group, had been based in the North-East, moved to London. At the same time, I also returned to London from Edinburgh, and was asked to join the editorial group. It is an indication of the degree to which I had been absorbed into the wider issue community, and also of the degree to which my intellectual detachment and sense of priorities had become confused that I accepted, and not only because of the opportunities for "fieldwork". However, in the event, the experience was very helpful in understanding the complexities of

the relationships and boundary disputes between the disciplines which participated in both the unemployment and health debate and in the journal.

During the whole period of time Summer 1983–Summer 1985 when the majority of the interviewing was carried out, I attended 9 meetings, seminars or conferences of half a day or more in length where “unemployment and health” was either the sole topic or a major one, making detailed notes. Apart from “what happened” at these gatherings, it was interesting to observe the extent to which there tended to be at least some members of the “core set” (Collins 1974, 1981) in the debate present, so that by mid-1985 this group had taken on somewhat the appearance of a travelling circus. New faces appeared to give papers on, say, the historical aspects of the debate, but the question of the effect of unemployment on PHYSICAL health was nearly always dealt with by a small group of “authorities”, and if they could not be obtained, members of another small group of “accredited” spokespersons. This, then, became another process to be explained, and, in turn, against which to test the model I was developing, that is, the persistence over time of certain individuals (and only these) in the debate.

I also continued to visit the research unit where I had originally worked, and to have informal discussions with members of the unit about the stage at which their own work on unemployment and health was to be found. The fruits of all this, (as will be familiar to all ethnographically inclined field workers) was a mass of papers, fieldnotes, interview notes, newspaper cuttings and other “documents” such as pressure group minutes, correspondence between participants, and working drafts of research papers. This “mass” then had to be distilled into categories through which the progress of the debate and the participation of “core” members of the issue community could be understood.

## **2.3 Between description and analysis**

Silverman (1983) uses the concept (drawn from cognitive anthropology) of the “formal ethnographic description”, by which is meant an “adequate and replicable account” of a routine social practice within a given cultural context. This is what I hope to establish for the categories used in this study. Material was selected for inclusion in the substantive chapters according to how well it seemed to illustrate the ways in which different theoretical approaches led to



the establishment of these categories of action within the various groups involved in the debate. In the course of exploring the case-study from these different perspectives, an attempt will be made to establish the usefulness of several equivalence categories, such as:

- "professional entrepreneurialism" as defined by Aronson
- "cycles of credibility" as applied by Latour and Woolgar to **scientists**, but extended in the present study to members of all the occupational groups involved
- "information subsidy" as defined by Gandy
- the "technical" and "moral fragmentation" of an issue
- "interesting (interessement)", "translation", "enrolment" and the attempts to establish "obligatory points of passage" as defined by Callon (1985), Law (1986) and Latour (1987)

The overall aim of this exercise (which might be characterised as an "analytically inductive case study" (Katz 1983, p. 133-134)) is to construct a "model" of the form and conduct of policy-relevant scientific (or technological) debates which other investigators may attempt to apply "deductively" (in Heclo's terms) to future cases. The search for categories by which to classify patterns of activity within the unemployment and health debate was carried out in pursuit of this overall goal. It is of course more than likely that future case studies will not find the "model" proposed here to be a useful guide. When this happens, not only the "model" itself, but also the methods used in its composition, and the "accuracy" of account, will be questioned. This close relationship between "reliability" (descriptive accuracy) and "validity" (theoretical adequacy) is a normal and desirable aspect of the process of inquiry.

Plummer has observed that: "the move from data to theory" is perhaps "the most difficult task in all social science research .. the moment of intellectual imagination when data turns itself into generalisation, concepts and theoretical hunches." (p. 127). Plummer offers no exemplary case to follow. It is for this reason that the present study is presented as a "model-building exercise". That is, the reader will be invited to judge the accuracy of the empirical work and the validity of the general concepts derived from the material by applying them, in a necessarily preliminary way, to other cases. Can I explain to a reader what an information subsidy is well enough for it to be recognised in other debates? Or an example of technical fragmentation? If so, then I may be granted to have provided a "formal ethnographic description" in the sense used by Silverman, i.e. a replicable account of a routine social practice. The

patterned collection of such practices may be seen as the "model" of (in this case) policy-related debate.

Within the present study, categories were arrived at by means of tentative formulation which emerged out of the interaction of "working hypotheses" with the data gathering process. This was followed by "testing" during the next phase of fieldwork. Concepts and categories were retained as long as they appeared to make sense of subsequent events and subsequently gathered accounts. Those which remain and are used in the analytic account of the debate presented here have withstood these tests. Of course it is always possible that if I had followed the debate for a longer period, or spoken to other people, some or all of the categories would have had to be discarded.

I will offer two such "tests" as examples, as they were, almost in the full positivistic sense, "crucial experiments" in relation to my latest semi-formulated hypothesis about what was "really going on". One (the first) took place in the autumn of 1984. I asked someone I had just interviewed (we were in a long lunch queue in his college canteen) "What are people like yourself [economists] doing in this debate? You seem to have defeated the opposition and yet you continue researching the topic?" "Oh" he replied, smiling, "because we can do it better than they can." I asked what he meant. "You don't need a medical degree to do health planning" he elaborated. His remark confirmed what had been a vague, but growing suspicion, that the debate was not about what it seemed to be about at all, and that I must explore it in terms of professional boundary disputes.

The second crucial experiment took a great deal longer. For three and a half years I kept books of newspaper cuttings which I thought would be relevant to the debate. At first all of these were about factory closures, mass redundancies, parliamentary debates on unemployment, unemployment figures, and the like. Not once did any of these provoke a high-profile media discussion on the health of the unemployed. Subsequent to its very early appearances, almost all the media attention given to the unemployment and health debate followed revelations about "cover-ups" or "misuse of government statistics", and was usually the result of some form of "leak" by a scientist working in or close to government who was infuriated, not by the unemployment level, but by the threats to professional position and integrity posed by government policies towards the production and use of official information. To me, this

once again demonstrated the importance of the strategies of various subgroups within the different professions and disciplines involved in health policy and planning in the process by which "the health of the unemployed" was produced and sustained as a "social problem".

Having reformulated my own expectations in this way, a return to field notes and interview material seemed to reveal entirely new things, which I simply had not perceived when looking for a purely "political" or "administrative" or even "scientific" phenomenon. I had been totally unable to understand why the participants in the debate did the OTHER things that they did, although I had tried to read as much as possible of their previous and subsequent work. I had been unable to understand the lack of any consistency in their political allegiances, or any correlation between these and their positions in the debate.

So, in answer to Heclo's points on the importance of a theoretical approach to case studies, the present one began with an approach which proved unsatisfactory, passed through a series of others which were partial improvements, and came eventually to one which seemed to allow me to understand more of my material than any of the others taken singly. This came about by almost a laboratory-bench "tinkering" (as described by Latour and Woolgar.) Each attempt at an explanation left anomalies. The anomalies lead to a further search for alternative or additional forms of explanation, until at last most of them had been satisfactorily incorporated. What results is by no means presented as "final", but merely as a fruitful starting point for future work, which has perhaps cleared some undergrowth and suggested some pathways.

## **2.4 Action and context**

Having attempted to deal with Heclo's question "A case of what?", and to offer an account of how categories and organising concepts have been arrived at in this study, I must now turn to the problems involved in making claims about "causation". Silverman (1983) has criticised the tendency of micro-level research, for example, in health care, to ignore the wider context, in this case, of changes in political ideology, economic climate, and administrative priorities. And yet, Hammersley and Atkinson warn against merely setting an ethnographic study against some wider background, leaving unexplicated what the author

thinks are the connections between the two. Similarly, Latour and Callon and Law have criticised the use of "background" concepts such as "interests" in an unexplicated fashion, as "factors" available to the sociologist and not to actors themselves (see Ch. 1). However, Whitley (1980) states:

In making sense of scientific development in the contemporary sciences, the context of research is obviously a crucial aspect and variation in its structure can be related to variation in research practices...

As far as possible, I will treat "causes" and "wider influences" to be just what participants say they are. This procedure is also "true to" my own moral career as a participant observer, as I found myself drawn more and more into the wider concerns of other participants in the debate in order to make adequate sense of their own activities. For example, what had unemployment and health to do with resource allocation in health planning? Or energy expenditure in overweight women? Or the outcome of pregnancy in working mothers? By the very effort to understand the ways in which my participants DID participate in the debate, I (like them) came up against the wider context in which they operated and learned to account for what we were all doing there. I learned about the "causes" of actions by learning how participants made sense of what they were doing. This type of causality, cause-as-reason or motivation, can perhaps be left for the moment, at the level of members' accounting procedures.

However, I am also going to make statements about the unacknowledged background and unintended consequences (to borrow Giddens' use of the phrases, for example, in Giddens, 1984) which gave the unemployment and health debate its shape during the period I observed it. In this, I will depart from the strict letter of the ethnographic method explained in section 2.2. in making use (at least by implication) of "causal" language. It will not therefore be possible to avoid discussion of the problem of "causality".

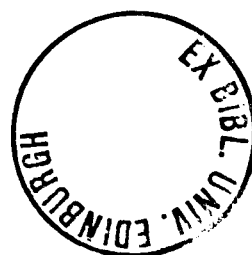
Many authors have tackled the problem of the difference between historical and sociological accounts, and of the ways in which the concept of "cause" is used in non-experimental research situations. By their nature, case studies are post hoc, and the researcher is open to the accusation that she has constructed an explanation back-to-front, when the outcome was already known, and failed to meet the requirement that the account be subjected to the

test of prediction. One is also vulnerable to the accusation of practicing "Whiggish" history, which sees the whole process as somehow teleologically oriented towards a certain outcome. Is it legitimate to use concepts such as "cause" and "effect" when one is not in a position to carry out replications, comparative case studies, or to make testable predictions?

In his discussion of the use of notions of causality in historical studies, Quentin Skinner (1966) concentrates on the sort of explanation that takes the form "idea 'x' influenced or caused event(s) Y, Z, ...". What is the nature of (even potentially falsifiable) claim of this type? Is it a claim by the historian to have discovered some unknown linkage? In which case the positivistic requirement that we demonstrate "constant conjunction" (a practical impossibility in most historical work) seems inescapable. Or is it a claim to have discovered some "inner relationship" already testified to by personages in the story themselves? The latter interpretation raises the question (familiar to sociologists) of whether informants' reports are "topic" or 'resource'. Skinner prefers to regard informants' reports of "influence" mainly as topic, and not as conclusive evidence. "It provides at most a clue to the intellectual biography."

There are, however, certain thoroughly mundane tasks which must be seen to have been carried out by the researcher who makes even such a cautious use of the concept of "influence". He or she must know, for example, whether the 'influenced' person (call him or her P2) actually had contact with the source of possible influence (P1) or with P1's writings. Was this contact temporally prior to the actions we seek of explain? In the present study, comparison of documentary evidence and interview material had to be carefully carried out in order to deal with these questions. But even so, as ethnographers of science have shown, claims of influence can serve as a form of 'persuasion', as do references in scientific papers (see Gilbert 1977, Law and Williams 1982), or a way of signalling allegiance or membership of a valued group (as acknowledgements in academic papers often seem to be).

A more common use of the concept of 'influence', Skinner feels, is as part of a claim by the historian to have "discovered" and established by her own investigation an "inner connection between P1 and P2"(p. 206). However, he is also sceptical of the possibility of establishing such a statement on a firm basis, and feels that this is a particularly acute problem in the history of science.



The evidence about intellectual relationships arising out of attempts to solve puzzles is usually impossible for the historian to recover, and tends in any case to be of [a] systematically misleading kind.(Skinner 1966, p. 207)

The history of an idea or event, Skinner continues, can never be adequately written in terms of its leading actors.

The appropriate strategy must then be not to begin by abstracting leading ideas or events, but rather by describing as fully as possible the complex and probably contradictory matrix within which the idea or event to be explained can be most meaningfully located.

I have found it most important to be clear that the unemployment and health debate was neither an account of the ideas of "great men and women", nor one of "reality out there" being slowly "discovered". The whole point of treating a debate whose outcome was, at the beginning (and the end) of fieldwork, indecisive was in order to be forced to concentrate upon the social processes which shaped the debate and its interrelationship with the political process. In Dreyfus and Rabinow's words "far from being differentiated *by* their objects, discursive formations [written or spoken discussion] *produce* the object about which they speak." (p. 61) The aim of the present account is to attempt to see the different ways in which "the health of the unemployed" was produced by the various discourses upon it: in different academic disciplines, in Parliament, in the DHSS, in pressure groups, in political party campaigns, and the ways in which these discourses interacted with attempts by policy-makers to take and to justify practical decisions.

However, the historian, sociologist or ethnographer still faces the problem of characterizing the relationships between context and ideas. She can attempt to increase the density of the account, from a potentially inexhaustible store of "evidence". This at least deals with Skinner's criticism that the main themes of a writer's work may not even be unambiguously discernible by a later commentator not immersed in the same context of meaning. When relating the work of different writers, Skinner insists: "It must be at least equally plausible to suggest that a writer's influence could derive from a chance remark, or even a misunderstanding of a remark, being taken up and developed ..." But, he adds, "The evidence required to supply any single convincing explanatory account is thus impossible to recover ..." (p. 210) Some of these difficulties may be partly overcome when studying a contemporary situation by close

observation, but these are still very serious problems in case study research. Latour and Woolgar (1979) found in their participant observation of a scientific team, that "Formal communication is the exception" as a major source of new ideas, and that "most published papers are never read, ... and the [rest] are transformed and misrepresented by those who read them." (p. 252) Fortunately, the rules of method adopted here will prevent us from assuming anything about the relative importance of informal and formal communication and allow us to explore the ways in which both formal and informal networks both promote certain kinds of "influence", and affect the specific kinds of misreadings and transformations to which communications are subject. However, the decision as to what will count as a 'sufficient' amount of evidence is not a simple one. It invokes unexplicated ideas about proper procedure in research, which is precisely what we are trying to clarify. Andrew Pickering (1980) asks

Having cast doubt on the accounts produced by scientists, what are we to make of our own data when we read scientists' writings and talk with them in interviews?

he answers:

I am well aware that I have not solved the problem

Skinner appears to conclude from his deliberations of these problems, that the historian should pretend to no more than description. A very dense historical *description* he argues, has the same status as certain types of scientific explanations which consist of "the most precise correlation between all possible variables" (1966, p.214). He also justifies this emphasis on "complete description" in terms of intellectual humility: "historical explanations attempted in this model would be bound to look much more provisional than is usual in the history books" (p. 215). Such a commitment to the textured account comes close to the anthropological concept of 'thick description' (originally formulated by Ryle, see Geertz 1983 p. 51-53). Emerson (1983) quotes the anthropologist Edmund Leach's characterisation of "thick description" as

the intricate interweaving of plot and counterplot (in Emerson 1983, p. 28)

'Thick description' has also been recommended by sociologists of scientific knowledge (see Bijker Hughes and Pinch 1987).

However, this argument is problematic. Firstly, Skinner is calling for complex description on the one hand, and on the other acknowledging that historians' language is "commonsensical" and not even "sufficiently rigorous to establish agreement on descriptive categories" (1966, p. 214). Secondly, these "descriptive" methodological strategies, as applied to social history, have been criticised by Stearns (1985). who feels that:

there is a serious weakness in this new narrative argument ... The new narrative.... does not provide a means of coherently describing or explaining social change. Social historians ... must not be trapped in stylistic choices that unduly weight the scales against the possibility of ... dynamically linking the present to the past. (p.324-5)

or, one might add, action to social context.

Stearns would deny to social historians "the luxury or simplicity of an event-based narrative". The narrative cannot stand on its density or texture alone, and should rather, he argues, be used for illustrative purposes and not as the basic structure of an account. Similarly, Joynt and Rescher (1961) insist that the historian must undertake, at the very least, such classifications as are involved in describing events as "wars", "budgets", etc., "which can only be attained by grasping the relationship of causal and conceptual interrelation among the chronological particulars." This neatly expresses the inescapability of concepts of "explanation" and therefore of "cause", even to ethnographic accounts.

In the present study, categories are offered as "formal ethnographic descriptions", and "causal" statements are proposed as working towards a "model" which can be more widely applied (as advocated in Frake 1983 p. 66-67). It is on the fruitfulness of these categories and generalisations for further work that claims to "validity" and "accuracy" in the substantive account will ultimately rest. In Latour and Woolgar's words:

the degree of accuracy (or fiction) of an account depends on what is subsequently made of the story (Latour and Woolgar 1986, p. 284)

To further paraphrase these authors, like the fate of statements about the "effect of unemployment on health", the fate of the present account of the debate rests in the hands of its readers. My appeal that others "test" the applicability of the approach used here (to destruction if necessary) to further our understanding of policy-related academic debates is itself an "enrolment"



strategy, an attempt to establish a “point of passage”. In chapters 4 to 9, the concepts established in chapter 1 will therefore be applied, with the aid of the methodological principles and procedures outlined in this chapter, in an attempt to construct a coherent and “plausible” social history of the unemployment and health debate, in such a way as to suggest an approach to examining other policy-related scientific debates. First, however, chapter 3 will outline the scientific and administrative changes that preceded the unemployment and health debate, as a way of showing what the potential was for “inter-esting”, “translation” and “enrolment” by actively entrepreneurial groups and individuals.

**CHAPTER 3**  
**EPIDEMIOLOGY, ECONOMICS, AND HEALTH**  
**POLICY DEBATE**

### **3.1 The rationalisation of health**

The "Unemployment and Health debate" unfolded against the background of two related sets of institutional changes which took place in the early 1970s: those which were intended rationalise public expenditure on health, and those which produced a form of knowledge appropriate to this rationalisation. There was also an increasing emphasis on the need for planning, evaluation, efficiency and cost effectiveness in all other areas of public administration, in an attempt to control what had come to be seen as runaway growth in government expenditure. The emphasis on planning, in turn, gave rise to perceived needs for different kinds of information about the health and welfare of the population and the provision of services, and for new kinds of experts to collect and analyse this information. At a later stage, following the election of a Conservative government in 1979, the Keynesian paradigm of economic management was swept away and replaced by a "Friedmanite" ideology of return to free market principles. This major policy shift resulted, amongst many other things, in the acceptance by a British government of over 3 million unemployed by 1984, as well as the increasing incursion of market forces ("privatisation") into areas of welfare such as housing and health.

Even during the period of expansion and "welfare consensus", many of the postwar welfare measures against the "five great evils" of want, squalor, idleness, disease and ignorance had given rise to concern about cost and effectiveness. The principles of social insurance did not really work for many of the groups most vulnerable to spells of inability to earn. "Full employment" was never a reality for women and the disabled and swiftly became less of a reality for other groups such as older workers in heavy manual jobs (Reubens 1970, Walker et. al 1985, White 1983). Because of the persistence of un- and underemployment, and of low wages in some sectors of industry, poverty was far from abolished (Jordan 1987, p. 99-103). Squalid housing of a new kind was invented, which was to prove as great a threat to mental hygiene (and even to child health and life expectancy see Fox and Goldblatt 1982, Martin et.

al 1987) as the old back-to-backs had been to physical health. Elderly people, preserved from death by the advances in medical technique and chemotherapy, survived into materially and psychologically deprived extreme age.

One result of these developments, it may be argued, was that a Health Service which had never been intended to do so (D M Fox 1986, p. 45-49), inherited responsibility for social distress in general, both at the practical and at the ideological level. This phenomenon has been termed "medicalisation" by many writers, a term often used vaguely and polemically, but which can be given more concrete meaning here in relation to the history of post 1950 British welfare provision.

The acceptance by the new school of "biographical" medicine in General Practice (documented by Armstrong, 1983; Jefferys and Sachs 1983) that patients' fears, sadness and loneliness were not "trivia", but legitimate business for doctors to deal with, was both a part of this phenomenon and a factor which accelerated its growth. The entrepreneurial activities of the Royal College of General Practitioners after its formation in the early 1960s were extremely successful in restoring the status and command over resources which had been denied them by the hospital and consultant-orientation of Bevan's 1947 reforms. A result of this, however, was that increasing numbers of "social problems" began to be dealt with by the "family doctor" as forms of individual pathology.

It was, therefore, quite likely that at some point, some section of the medical profession would come to consider that the effects of social and economic policies upon whole communities were also legitimate medical business. As part of the 1970 re-organisation of social work (the Seebohm reforms), the Medical Officers of health lost control of local authority social workers, who sought greater professional recognition and autonomy. (The causes and effects of this are now beginning to be disputed by social historians, see Jefferys 1987, Lewis 1986a). The group whose status had been radically undermined by this change, and by the 1974 Health Service reorganisation, the Medical Officers of Health were then re-designated "Community Physicians", and an ongoing debate began as to their role and domain of expertise. A strategy pursued by some sections of this new speciality was to take an interest in the social health of communities (complementary to that of individuals). This, however, was not the only reason

why sections within Community Medicine took up the question of whether unemployment and other forms of social deprivation, affect health. The question was posed within a context where, increasingly, a debate was being conducted on the "determinants of health". This debate was part of a challenge to the hegemony of the high-technology, high-spending, curative branches of medicine, and thus brought social/community medicine into an alliance with interests within government.

The history of the British NHS has been accompanied from its inception by debates on expenditure. In the early 1950s, R. Titmuss and B. Abel-Smith re-analysed figures from the Guillebaud Committee Report to show that the "crisis" of rising costs was largely an artefact of inflation (Ham 1985, p.19). A short period of growth in the Service which followed Guillebaud overlapped with the adoption by British government of American style operational research techniques, and these techniques, although not introduced for these reasons alone, continued to be used in the attempt to cope with the economic constraints which began to be more strongly felt in the mid-1970s (Butler and Vaile 1984, p. 78-79; Ham 1985 p. 93-94). In 1968, the Labour government, in the wake of devaluation, "took a serious look at controlling the size of the public sector and converted the small steps that had followed since the 1961 Plowden Report into the powerful PESC (Public Expenditure Survey Committee) system" (Heclo and Wildavsky, p 274). During this period of administrative reform (Fry 1981), it was discovered that "Ministers lacked sophisticated information and analysis such as that used in US programme budgeting. The result was that resources were being allocated without much reasoned deliberation" (Heclo and Wildavsky p 268).

Heclo and Wildavsky continue:

By 1970 it was clear to many Treasury officials that PESC needed further development and that this should entail increased effort at evaluating individual programmes and their results. (p 274)

The impact which this had on the Department of Health was stated by a DHSS official, H.C. Salter, at a seminar on health economics held at the University of York's new (DHSS funded) Health Economics Unit in 1970.

One might expect during Public Expenditure Review that there should be some scientific judgement of priorities and of the merits of cutting one programme instead of another ... But at present there are no means of arriving at scientific assessments ... We are very conscious that in this new approach, we [Health

and Welfare] may well lose in the struggle for a proper share of the national cake if we are not able to present our case more scientifically (in Hauser (ed.) 1972, p. 18–19).

The participation of Abel-Smith, an economist, in the early efforts of the Guillebaud Committee to identify the causes of “cost inflation” in the NHS and impose “priorities” and criteria of “value for money” acts as a reminder that economists were involved from the beginning in these exercises. In 1970, the DHSS introduced

an output budget for health and personal social services, which would routinely relate expenditure more closely to objectives (Banks 1979, p. 154)

1970 may be also regarded as marking the establishment of “health economics” as a distinct subdiscipline in Britain, centered around the York unit. (Porter 1979, and for sociological observations on the work of this research group, see Mulkay et. al. 1987a and b).

Questions of cost and efficiency had not, however, been ignored by the medical profession. In the 1960s, practitioners of “social medicine” turned their attention in this direction. The most well known results are to be found in the work of A L Cochrane and J N Morris, who combined skills in epidemiology with concern for health service organisation. The first edition of Morris’ book “Uses of Epidemiology” was published in the mid-sixties. It laid emphasis on the importance of measures to promote health rather than merely to deal with illness once it had occurred (an approach which came to be known as “the upstream model”). “Prevention” was held to be of increasing importance, in a period where the pattern of disease was seen to be changing to one where most illness was caused by chronic conditions which had long (and expensive) natural histories and were, basically, incurable. In 1972, Cochrane’s 1971 Rock Carling Lecture on “Effectiveness and Efficiency: Random Reflections on Health Services” was published as a book (Cochrane 1972). It is perhaps still the most influential book written by a doctor on the subject of health service organisation.

Morris used the idea of “community diagnosis” to refer to the need to monitor the state of health of a population in order to build up a picture of “need” which would allow the adequacy of health care provision to be assessed. Cochrane’s notion of “effective” treatment was one which “altered

the natural history of disease" (for the better). BOTH concepts required the monitoring of health status outside hospitals (Acheson 1968). This was to have been part of the task of the new profession of Community Medicine (see, for example, Gill 1976, Holland 1982, Morris 1971). Epidemiology and social medicine were less in demand now for the control of infectious disease and other traditional functions of the Medical Officers of Health (Florey and Weddell 1980, Parston 1980). However, they could seek a new role by developing links with the planning and administration of health services as part of the drive toward rationalisation of the state sector which took place in the late 1960s and early 1970s.

Up to the early 1970s, the health of persons outside the hospital or health system was the preserve of "social medicine". But by 1970, it appears that at least some people within the DHSS foresaw the need to be able to make their claims against other Departments in the annual battle with the Treasury "scientifically respectable". The disciplines to which officials looked for expertise appropriate to their claims were both "social medicine" and economics. Health economics had been flourishing for some time in the USA and one of the largest health-economic studies ever undertaken in the UK was Feldstein's in the 1960s (Feldstein 1967). However, to some extent, the DHSS can be said to have further promoted the discipline, by funding the York Unit specifically to train up new members (the M.Sc. course in Health Economics had its first intake of students in 1977). The exigencies of the struggle between a spending Ministry and the Treasury began to create an intellectual space into which entrepreneurial subdisciplines could move (for an explicit debate amongst economists on this point see Engelman 1980, Akehurst 1981). This is one example of the importance of the relationship between academic and administrative concerns in the development of an emerging discipline, and constitutes one side of the essential background to the unemployment and health debate.

Another development which formed part of the early social history of the debate was the reorganisation of the funding of medical research which took place during the 1970s. In 1971, like all other departments funding research, the DHSS appointed a Chief Scientist. The first Chief Scientist, Sir Douglas Black, elected to play a part-time, advisory role, and worked with a "Chief Scientist's Advisory Committee" (CSRC) of experts, whose role was to assess priorities across the whole "research programme".

Within the DHSS at this time, broadly, four major areas of "R&D" may be distinguished: Health Service, Personal Social Services and Special Client Groups, NHS Equipment and Supplies etc, and Social Security. Additionally, large amounts of money went to fund studies carried out by the Medical Research Council (MRC). The balance sheet in the period under study looked something like this:

TABLE 1

DHSS R&D Expenditure 1976-1981

(£000s)

	1976/7	1977/8	1978/9	1979/80	1980/81
Health services	3684	4134	4364	4865	5785
PSS/Client Grps	2594	2887	3456	4246	5326
NHS Equipmt.	2936	2614	3118	4196	5399
Soc. Security	270	329	352	353	469
MRC	8936	8920	10740	11436	13778

Source: DHSS Research and Development Handbooks

The tables were not published after 1981

The MRC is an independent research council whose funds come directly from the Department of Education and Science's "science vote", and are not allocated on a "customer-contractor" basis. Its members, the key decision makers who allocate both its DES and its DHSS grants, are drawn from the "high-tech" clinical specialities, proud of their independence and high scientific standards. In the early 1970s, as part of the "Rothschild" re-organisations of government-funded research (Kogan and Henkel 1983), 25% of the MRC's allocation from the DES was transferred to the DHSS to be devoted (on a contracted basis) to "Health Services Research". By this was meant research directed at the "effectiveness and efficiency", appropriateness and acceptability of clinical services, especially services to the less glamorous client groups such as the mentally and physically handicapped. These services were a traditional area of concern to "social medicine", and the composition of the CSRC's medical membership reflected this -- the Chief Medical Officer, Sir Douglas Black later chaired the group which produced the Black Report on Inequalities

in Health (DHSS 1980). Black himself had been a leading clinical scientist, and a member of one of the MRC Research Boards. But the committee included Cochrane, Morris and another prominent figure in social medicine: Prof Alwyn Smith (an early academic participant in the Unemployment and Health Study Group). Other members during the brief life of the CSRC included economist Alan Williams, and strong representation of social policy, including both David Donnison and Peter Townsend at various points in time. Only three "pure clinicians" (out of 18) were included in 1973, this rose to 4 (out of 19) by 1979, the last year of the CSRC's existence.

Sir Douglas Black expressed enthusiasm at the composition of his Research Committee:

We were lucky in the help we enlisted from outside scientists. It was a totally different set of disciplines from those [which] one thinks of in connection with clinical research; for example, sociologists, economists, statisticians and so on. It was an interesting experience, and not merely in the Chinese sense of the word (Black 1986).

Thus the composition of the CSRC can be seen as influenced by the critique of the dominance of the clinical specialties in medicine, both in terms of intellectual hegemony and in terms of command over resources. "Clinical" dominance was regarded by many policy makers as acting to the detriment of other services, particularly under the new conditions of economic stringency. This critique united two separate interests: the concern to limit health expenditure without arousing public protest, by discrediting that section of the medical profession which pressed for more and more resources for advanced technology, and the "radical" rejection of mechanistic forms of medicine, which originated outside of government. During its short existence, the Chief Scientist's Research Committee of the DHSS expressed the claims of social/community medicine to set new priorities for health.

In Black's view, it was the restrictions on resources and civil service staffing that resulted from organisational reviews which hindered the effectiveness of the Rothschild machinery in the DHSS. Additionally, however, as also discussed by Kogan and Henkel, there was the inherent conflict between politically determined priorities (in this case, services to the "priority client groups" of older people and people with disabilities) and what Black termed "the question of scientific opportunity". In other words, how to strike the balance between



unglamorous, service oriented research on the elderly, disability and problems of service delivery and those topics which "good" scientists wished to pursue in order to gain prestige in the eyes of peers? This was made even more difficult by the fact that even in the case of service-oriented research, it was not easy to "identify a customer". According to Black, the most likely candidate was

An Under-Secretary in charge of 18 or 19 Divisions in the DHSS ...They vary in their devotion to the furthering of research, and given that research is only about 1% of their total budget, this is only natural. ... Bureaucracy arose from the necessity to identify a credible customer.

In 1978, Sir Arthur Buller took over as full time chief scientist and the CSRC was dismantled<sup>10</sup>. In 1979 all DHSS funded research units began to be reviewed with a view to cut-backs. At the same time, the MRC's role was once again recovering strength. In 1981, the funds transferred to the DHSS were handed back, on the condition that the MRC undertake to fund more research decided on the basis of "need" rather than judgements of scientific merit alone. In order to facilitate this, a Health Services Research Panel was set up within the MRC. This arrangement was termed the "Concordat".

### **3.2 The bureaucratisation of "Public Health"**

By the late 1970s there were two sources of discontent within community medicine and the disciplines to which it acted as a "client" or customer profession. One was dissatisfaction with the waning of the role which social medicine had begun to play in determining priorities for health research funding at the height of its influence on the CSRC. The other was the increasing bureaucratisation of Community Medicine, which had been seen at one stage as social medicine in practice.

In the course of the 1974 re-organisation of the NHS, the position of Medical Officer of Health had been abolished. The functions of the MOsH were combined with those of the Medical Administrators of the old Regional Hospital

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<sup>10</sup> For an account of this see Ashworth 1984, p. 32

Boards, and the medical staff of the university departments of public health and social medicine, to form the new speciality of Community Medicine. The function of these specialists was to investigate and assess the needs of the population so that priorities could be established for the promotion of health and the prevention of disease, as well as the provision of medical care (Report of Joint Working Party 1971, quoted in Garraway 1984, p. 15, see also Scottish Home and Health Department 1973).

In the words of the 1972 Report on NHS management known as the "Grey Book" (DHSS 1972), community physicians were seen as "clinical managers". They were granted consultant ("specialist") status in order that the voice of management should have a degree of legitimacy equal to that of the high-spending hospital clinicians on the consensus teams at the various decision-making levels of the health service (for an account of the problems created by this reform, see Russell 1984, Lewis 1986b).

The result of this was, in the eyes of many both inside and outside of Community Medicine, the worst of all worlds. The role of the MOH in protecting and improving the environment (both physical and social) took on something of the nature of a "myth of the golden age". In the words of Sir John Brotherston, a past Chief Medical Officer for Scotland:

The pioneer Medical Officers of Health had many obstacles to contend with such as primitive organisations and attitudes, but they could range freely in their concerns over the whole front of human well-being ... (in Draper and Smart 1984)

Dissidents regretted the loss of the charismatic public health role, (described colourfully as "Gotterdammerung", for example, in Acton 1984, and as "death throes" in Smith 1979) and felt that community medicine had moved too far towards a purely managerial function within the hospital sector. Health Authorities, unlike local authorities, are "quangos", appointed ultimately by central government. Many within Community Medicine (and all who became involved in the unemployment and health debate) regretted the loss of accountability to an elected body and of the "professional advisory role" which this entailed.

There was no shortage of sources of legitimating knowledge claims for this position. Epidemiological research had for some time (embodied chiefly in the work of McKeown and his collaborators, see McKeown and Lowe 1966,

McKeown 1976, 1979), been "demonstrating" that the major determinants of health lay, not in clinical medicine, but outside, in the standard of living and life-style of the population as a whole. An expert advisory role vis-a-vis the local authorities, who are responsible for housing, transport, social services and environment, was therefore regarded as preferable, both in terms of political structure and of the possibility of taking necessary actions to promote health. It was therefore ironic, in the eyes of many commentators on health policy, that at this of all times, the "community" physician should be more than ever tied to management of clinical medicine.

A loosely connected faction within mainstream community medicine was forming around these discontents in the early days of the unemployment and health debate. Taking some liberty, I will refer to this tendency -- a set of ideas rather than a stable group of individuals, as "Manifesto" community medicine. The "Manifesto" document "Re-Thinking Community Medicine" (USHP 1979), was produced by the Unit for the Study of Health Policy, a Foundation-funded policy and research unit, in 1979. The journal *Radical Community Medicine* began to appear in the same year. Members of the USHP were the promoters, in Britain, of the ideas of M. H. Brenner; and the editor of *Radical Community Medicine* became co-ordinator of the Unemployment and Health Study Group, the major pressure group in the debate.

Like Aronson's entrepreneurial nutritionists (Aronson 1982a and b), the "manifesto" community medicine group aimed to influence both the mainstream discipline's major client, the Department of Health, and the content of their discipline itself. The entrepreneurial activity of USHP was directed towards the DHSS, and, simultaneously, at the "Community Medicine Establishment" (in the words of one of its senior staff). USHP attempted to promote a new role for public health and its practitioners within the re-organised NHS, and also in wider political and economic debates. In order to advance these claims within the medical profession, USHP and its sympathisers attempted to mobilise the anxieties of the Department over excessive spending on "high technology curative medicine", and to direct attention to the social and environmental aspects of the prevention of ill-health and its associated costs (the "upstream model"). In order to influence government opinion, they directed their ideas on the social causation of illness to the political party in power, which might have been expected to be sympathetic. A notion of environmental causation was necessary to the Manifesto group, both to influence the content of community

medicine as a discipline, and to promote the claims of the new sub-profession to a more prestigious position within medicine as a whole. Thus did "Manifesto" community medicine's strategy have "consequences for both the definition of a social problem and the development of a ... discipline", in Aronson's words.

However, as the producer of appropriate knowledge for health planning in the late 1970s and 1980s, an environmentally oriented Community Medicine was then faced with a problem. In order to increase efficiency and effectiveness or indeed to establish these criteria at all as part of what a health service ought to be doing, a strong case could be made that it was necessary to develop indicators of need for, and outcome of, health care (Butler and Vaile 1984, p. 127-128). It was argued by some that clinical field surveys with longitudinal follow-up, or at the very least "data linkage" (as proposed by Acheson 1967), enabling individuals to be followed in and out of the health system, are necessary in order for such aims to be satisfactorily accomplished. Comments by both academics and administrators involved in the unemployment and health debate illustrate their awareness of this issue. There were two serious flaws to this argument in terms of the realities of policy making. One was that the cost of the necessary studies was so great (Butler and Vaile 1984, p. 127-128). The other was, as Heller (1978, pp. 68-79) points out, that the discoveries of such exercises might themselves cause political embarrassment, by revealing the size of the "clinical iceberg" (Last 1963, Hannay 1979) and its unequal social distribution. Planners needed to know about population health status, the longer-term outcomes of various forms of hospital treatments and so on, and yet this form of knowledge threatened to be, in public expenditure terms, potentially as explosive as in-vitro fertilisation, organ transplantation and dialysis. It was not so much a technically as an administratively infeasible exercise. As one Scottish health planner, T. Drummond Hunter, put it:

It is difficult to give advice to government if the advice you are giving is not what they want to hear ... there is a perpetual conspiracy here to tell people at the top what they want to hear

And the way this "conspiracy" worked, he pointed out, was via the reluctance of civil service principals to be the carrier of unwelcome messages to their

superiors, for fear of "conveying the impression that he [sic] is 'unsound'"<sup>11</sup>.

At this time, there was another energetic and entrepreneurially inclined group which also had a claim to provide the intellectual foundations for a drive to rationalise the provision of health and social services. Although not a profession of the same standing as medicine, this group's traditional relationship to government put it in a strong position. Professional economists hold a unique position within the British Civil Service (Booth and Coats 1978, Cairncross 1968, 1970, Sharpe 1978, C. Smith 1987). Despite the presence of members of other numerate disciplines such as statisticians and actuaries in British political administration, the economic advisers played a key role at the point in time when health economists outside government were launching their claim to a voice in health policy making (Bulmer 1986 p. 29, p. 200, p.292; Bulmer 1987 p. 16).

This position of influence was described to me by civil servants who became involved in the unemployment and health debate. One attributed the success of the economic advisers to their ability to "sell their wares" and "good leadership". This administrator explained:

It depends on the issue, whether I use economic advisers. But it also depends on the individuals involved. If I know that [a highly-thought of economist] is responsible for something, then I use the EAs[Economic Advisers]. But if there is, say, a statistician that I trust, then I'd use him, if he could do it. I need to consider questions like "If I need a quick-and-dirty on this one, will they stand on their professional pride and refuse to do it?" How do these people prove themselves? It's their UTILITY that is the crucial thing.

... I would say that in this Department the economists are now head and shoulders above the statisticians in the ways people like me use them. ... They deliver ... What I look for is perspective -- breadth -- and not being tied within one's own discipline.

And another commented:

We use the economists' and actuaries' advice a lot. ... I'm not sure where they overlap, myself. ... We also have to be aware of their professional jealousies. ... Generally -- can I be frank? --

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<sup>11</sup> However, that this is not a phenomenon solely of the civil service under Thatcher is witnessed by Opie's (1968) lament over attempts to integrate economic advice into the policy making process under Wilson.

the economists are better at giving answers. The actuaries want to take over the whole policy question. I get the impression the actuaries don't feel like civil servants at all. The EAs see the problems WE have in the way that WE [administrators] have them ... they can even anticipate the sorts of things we are going to ask before we do!

Like the adherents of the public health approach to health planning and policy making, health economists held the view that problems about the allocation of resources to the health sector would not be solved by "clinical management" alone, but only by strategies which extended over wider areas of social life. However, they differed from the public health view in two main respects:

a) they believed that health service planning and health policy could be better carried out by economists, because it does not require clinical training, but rather a greater sophistication in operational research technique,

b) they believed that the wider questions of "the determinants of the distribution of health and health-related behaviour in the community" (which they refer to by the economists' technical term, "production functions"<sup>12</sup>. could be more fruitfully discussed in terms of the use of models of consumer utilities to determine the "value of health" than in the public health tradition of Farr, Chadwick and Simon<sup>13</sup>.

For example in 1987, one participant in the unemployment and health debate put it:

as economists we hope we may be forgiven for venturing in where other disciplines have, perhaps wisely, refused to tread. OUR justification, or plea in mitigation, is that the investigation of the determinants of population health is in many ways akin to the estimation of production functions ... Economists have considerable ... experience with the estimation of production functions ... (Gravelle and Backhouse 1987)

Health economists play important roles in advising government departments, and health economists were, during the period of this study,

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<sup>12</sup> It should be pointed out that although they do not use the term, public-health oriented epidemiologists ALSO use "production functions" -- this merely refers to the hypothesised relationship between input and output. The important difference, symbolised by different language, is in what are thought to be the biologically and politically relevant FORMS of input and outcome.

<sup>13</sup> see, for example, Fuchs (1972) Perlman (1974), Wagstaff (1986a and 1986b)

swiftly establishing a more influential position in relation to the DHSS. Community physicians however, are marginal to medicine, and medical advisers to government have not traditionally been drawn from the ranks of practising public health doctors or community physicians. This rendered the relative positions of these two subprofessions more equal in the contest for intellectual hegemony over health service planning than might otherwise have been expected. Health economists can challenge the exclusive right of community physicians to the planning function in a way that, for example, physiologists and anatomists cannot threaten the hegemony of clinical specialists in hospital medicine. In particular, economic techniques such as "satisficing" and "sub-optimisation" fitted well into the new, increasingly bureaucratic style of health service planning, and the implementation of the Griffiths Report strengthened the hand of the more "managerially" oriented school of health economists still further (Ledwith 1987, St. George 1985).

An example of the confidence with which the claims-making activity of health economics was pursued in this period is provided by Culyer (1976):

Economic analysis of health service problems is ... no game for amateurs. The quack economist is no less a threat to society than the quack doctor ... [because] below the surface lies a vast body of highly technical, mostly mathematical work in the learned journals and scholarly monographs, ... Medical policy-making has been lumbered for long enough with a baggage train of ... amateur ideologues, amateur doctors --- and amateur economists (p. 9)

### **3.3 Social research under the "Rothschild system"**

In order to understand the ways in which members of the other two disciplines involved in the unemployment and health debate, medical sociology and medical/social statistics, participated in it, it is necessary to examine the ways in which the relationship of the social sciences (other than economics) to government has developed in the 1970s and 1980s. This also entails some discussion of the ways in which social science research is regarded, organised, and used in government departments.

Social scientists other than economists have always played a more marginal

role in government<sup>14</sup>. Accordingly, many of the institutions from which members of these disciplines participated in health and welfare policy debates were what Hall et al (1978, p.60) have called "intermediate bodies", and what Cherns (1979, p.45-46) calls "mediating institutions". These are the research institutes and policy units such as PSI, the Child Poverty Action Group and the Institute for Social Studies in Medical Care, which Hall and co-authors regard as situated between the more obvious interest groups such as trade unions and employers' organisations on the one side, and government on the other. In these intermediate bodies, of which the Unit for the Study of Health Policy was a perfect example, social scientists (including health-economists of less orthodox persuasion) collaborated with "social medicine" academics on questions relating to health policy and the health consequences of wider social and economic trends. The short history of the USHP stands as an example of the tendency noted by Hall et. al:

There is no firm boundary between research, pressure group activities and party politics. Altogether this "intermediate" area ... probably accounts for many of the initiatives and much of the pressure for change in social policy.

It may be that in the mid and late 1970's, the charismatic role which some groups within social medicine and public health felt obliged to adopt in opposition to the threats posed by the 1974 reorganisation fitted well with this innovative tradition of extra- institutional social science. However,

What we have witnessed in the past decade is a major shift in the relative positions of independent and [governmental] departmental research ... Government departments are now well provided with the kinds of data and expertise needed to counteract a challenge from an outside body, should they so wish. They are also in a better position to influence the type of research being conducted outside government. (Hall et. al., p. 79)

Hall et. al. date from the mid-1960s the feeling that the information needed for planning and policy making should be produced by Departments themselves. As early as 1956 the Guillebaud Report on the costs of the NHS recommended the setting up of a Research and Statistics Department in the Ministry of Health and thereafter the number of statisticians and economists in the department increased rapidly (Klein 1982, .p389). Between 1965 and 1975 the number of

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<sup>14</sup> for a recent and enlightening account of the position of "research officers" in government departments, see R Walker (1987).



professional statisticians employed in government increased from around 200 to 500. The DHSS, DoE, the Home Office and the Department of Employment all established their own small social research units (Bulmer 1982, p. 131). The statisticians, although dispersed around the various departments, had their own Central Statistical Office, part of the Cabinet Office (Moser 1973). In a similar manner, the Economic Section of the Treasury presides over the work of economists in the various branches of government. Members of both disciplines have "professional" leadership placed high in the civil service hierarchy (at Second Permanent Secretary level, see R Walker, 1987, p. 149), and centrally directed mechanisms for training and career development. Both statisticians and economists can enter on a "career grade" the equivalent of "Principal" (Grade 7) and benefit from the "fast stream" which replaced direct entry to the administrative principal grade. However, published reports of professional meetings indicate that many senior statisticians did not consider the careers open to government statisticians altogether satisfactory, despite these similarities in the career structures open to members of either profession<sup>15</sup>.

Sociologists' and psychologists' fates depended upon that of the units they worked for. A sociologist cannot enter the civil service via the "fast stream" directly into the grade-7 equivalent (Principal Research Officer), and will be subjected to the galling experience of seeing economists with equivalent (or fewer) qualifications begin at a higher level of seniority and remuneration. Statisticians were similarly "advantaged" in early career terms, but the view has been expressed by many commentators that only the economists end up with a truly coherent occupational or career structure with ready access to the higher reaches of the civil service (Bulmer 1978, p. 37-38, Bulmer 1987, p. 16, Booth and Coates 1978, Cherns 1979 pp. 52-54).

Research units within government departments hold an ambiguous position, and working in such an environment may entail a particular type of "cycle of credibility" for researchers, as explained by Prince (1983). Sociologists working in government, like economists and statisticians, are regarded as resources for administrative staff. However, there is no "Central Sociological Office", and no

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<sup>15</sup> for examples of such dissatisfaction and the continuing debate on statistical careers in government see, for example, Allen (1970), Griffin (1985)

centrally directed scheme of training and career development<sup>16</sup>. Sociologists usually work in specialist Research or Policy Units headed by a professional or scientific civil servant at the Undersecretary level. The Social Research Branch of the DHSS was one such unit. Prince comments that, in general:

Operating within regular government hierarchies means units have had to adjust to the administrative culture of public organisations, with its concern for risk avoidance, the short term and the practical. This context delimits the planning and research functions, making them specific to a particular set of legislative and administrative concerns (p. 117)

Bulmer (1982) goes further. He (like Drummond Hunter see section 3.2) feels that the constraint of "soundness" does not only enter into the career considerations of researchers working INSIDE government. As central departments took on more information- generating tasks, they also acquired a more important role in the funding of external research. After the Rothschild reforms in 1971, a "major change in the philosophy and organisation of social research" followed.

The evolution of this structure of government support for social research ... places considerable power in the hands of departments to influence the direction taken by social research ... The requirement that research should be related to an identified policy concern of a department serves to bind social research increasingly to CURRENT interests of SPECIFIC departments. It then becomes more difficult to develop research which deals with a problem that cuts across departmental boundaries (pp. 143-144)

The relevance of this analysis of the position of various kind of "expert" within government departments to the present study is reflected in the comments of two other civil servants. As one administrator explained:

[Unemployment and health is] not clearly focussed in terms of departmental responsibilities ... in organisational terms it doesn't make sense, so there's a bit of ad-hocery about it.

And according to another:

Unemployment and health is not at all a good example of how things can work together constructively. In fact, it's a particularly

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<sup>16</sup> though see Blume's (1987, p. 81) account of the brief life of the Social Research Co-Ordinating Committee in the Cabinet Office.

bad example ... In 1980, ... there'd been a lot of discussion -- it did have a group of administrators who were concerned with ... issues right across the department .... policy responsibility for that work would have lain with a planning department which had some of the most competent administrators around. ... So it had, beavering away at it, some of the best junior administrative minds. ... Unfortunately the impetus and expertise got shifted in the department because the policy group got radically changed and key staff moved on. So now there is no policy location for it

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And in any case, although

... unemployment and health was a very interesting intellectual problem, and we got hooked on it ... what could the DHSS DO about it? The problem was so enormous, and not in our remit. Unemployment and health is a hybrid ... it runs across two departments, two areas of responsibility. These issues that cross the pattern of organisational structures are much more difficult to handle successfully. It doesn't have an easy place in any client group oriented structures or service structures.

Prince (1983) points out that "Policy planners and researchers choose work topics that have senior official support and can be implemented ..." Criteria which government researchers (and, if Bulmer is correct, increasingly outside researchers too) must take into account when deciding which topics to pursue include:

... political implications, administrative feasibility, "quickies", "the unit can do it", avoid cans of worms, anticipate issues, and develop a distinct domain. (p. 151)

From which it can be seen that unemployment and health was always finely balanced in its chances of being taken up as a legitimate topic for research. In terms of cost, feasibility, the "can of worms" criterion and the distinct-domain criterion, the issue was a loser. Pure intellectual fascination fits neither the practical ethos described by Prince nor the post-Rothschild official commissioning process.

However, officials DID fear that perhaps just being up to date with outside research might not be sufficient to allow them to deal with media and pressure group activity, and this created a certain amount of pressure towards commissioning further research. These pressures are strong, as related by civil

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<sup>17</sup> for further explanation of these events see section 6.1.2.

servants involved in the debate: One spoke of the need to

... carry out the necessary sort of work for defensive briefing, for Ministers to defend their indefensible policies.

and another told of three days' hard work carried out by several high calibre administrative minds over a PQ which turned out to have been tabled by mistake.

### **3.4 Questions and answers**

Many participants in the unemployment and health debate remarked on the importance of Parliamentary Questions (PQs) in the process by which the health of the unemployed came to be seen as a "social problem." The importance of parliamentary questions in the day-to-day functioning of public servants, and the extent to which the need to answer them shapes their longer-term concerns is little discussed, although Butler and Vaile (1985, p.100) point out that the DHSS receives 5000 each year. Within the health-policy making community, it was common knowledge, and regarded as more or less routine, if not something to be advertised, that pressure groups, such as the Maternity Alliance for example, "briefed" MPs to ask questions. This practice was referred to as "planting a PQ". Stories also abounded of the friendly or "tame" civil servant who would help to frame a question "correctly", that is, in such a way that it could not be evaded by saying that the information was not available or too expensive to produce. The benefit to an MP of allowing himself to be "used" in this way was that s/he would be noticed, and noticed as capable of carrying out well directed probes of government policy. This is part of the "cycle of credibility" of the parliamentary politician. The desire to be "noticed" also meant that a question on the effect of unemployment on health might be asked as part of a whole string of others dealing with various aspects of health policy. For MPs, as for academics, unemployment and health was just one of many possible means to an end.

The civil servants whose job it was to answer PQs ("Principals") usually mustered and collated the necessary information from a variety of sources, including economic advisers, statisticians and other professional advisers, seemed rather ambivalent about them. PQs certainly have "nuisance value", and yet they were seen as a necessary part of the democratic process. As one

remembered:

Sometimes there are very important issues revealed and raised through PQs. There are a proportion of PQs that are hard to deal with. You've got all the rest of your workload on top of this. Some of them seem to be a sort of knee-jerk response to something that appeared in the Daily Mirror. Some of these MPs put down 13 or 14 at a time. It is a necessary evil.

Although I have mentioned the usefulness of the PQ to the careers MPs wishing to be noticed and/or to lay a special claim to one area of expertise, some questions seemed to lower civil servants' opinions of MPs, to the point where an administrator or economic adviser who was in sympathy with the aim of the question could feel "able to write it better myself". The problem of the collaboration between administrative and "expert" civil servants could be highlighted by a PQ. As one put it:

This is a classic illustration. Someone from the Actuaries Department once worked for me. He was perfect at doing these sort of totally neutral, objective appraisals. But he could not bring himself to carry out the necessary work for defensive briefing, ...

[MB: Is that really true, then, that you have to play to this sort of script? You make it sound like "Yes, Minister".]

Your first job is to defend your Ministers from making fools of themselves. You don't actually LIE, of course. The written PQ is the classic example of how to avoid telling the truth without lying, if you like.

On the role of the PQ in the unemployment and health debate more specifically, one DHSS official at least felt that such use of Brenner's work had

made the Department prick up its ears. I don't know that there was any departmental activity [on unemployment and health] before Brenner's work appeared. These analyses of Brenner's did lead to PQs being asked. It's that sort of thing that quite often happens -- either a piece of research or something a pressure group does.

However, another official told me that a different criterion was used when judging how accurately a question had to be answered when it originated from "outside":

There are different stages -- for example: "wouldn't it be nice

if--?" which could come from a PQ or an "outsider" report. For these, it's not too high on the policy agenda, so you work with broad orders of magnitude. If it's something that starts motoring and becomes a political option, then you have to get your data sorted out and do a proper analysis.

There was seen to be a distinction here between the roles of economic advisers on the one hand and those of statisticians and actuaries on the other. The latter would only be brought in once a proposal (whether it originated inside or outside of Whitehall) had given rise to a real option for policy change. Economic advisers, on the other hand, were a good source of rapidly produced estimates which might have wide margins for error, but could be used in the sort of rapid-response mode required for answering PQs. Ultimate responsibility was seen, however, as lying firmly with the administrator and not with any of the "experts".

I might ask ... for information, but I'd stitch it together myself. We don't let the professional support groups have responsibility for PQs -- only the administrators do that kind of thing.

Administrators will have three to five days to "get an answer together". In the case of oral PQs, a further challenge to the administrator's skills is presented by the threat of "supplementaries" In this case

The trick is to be able to guess what the follow-up questions are going to be. This is a test of your quality, whether you can do this.

The Parliamentary Question, then, plays a role in the cycles of credibility of both MPs and civil servants. In most cases, the asking and answering of PQs seems to be seen as a sort of ritual which is relevant to testing the "quality" of contestants, and which can also be a first attempt to put an issue onto the political agenda, or to return it there. They do not, in themselves, lead to policy action.

### **3.5 Appropriate knowledge**

It may be that in the case of health services research and planning, the trends discussed in this chapter (increasing emphasis on cost control, "bureaucratisation" of both research and service planning in the field of health) tended to force administrators away from social medicine and towards health

economics as the appropriate intellectual framework. Health economics could "produce the Black Magic rather than the Dairy Box" (in the words of a government statistician). Economists did not force their own views (either of the right answers or the right questions) on administrators, but at the same time accepted the role, when necessary, of "telling the customer what he really wanted".

As the role of social medicine/epidemiology declined in importance, as reflected in the history of the Chief Scientists Research Committee, the influence of the economists rose. At the same time, clinical/scientific hegemony was restored over medical research, but under conditions of increasingly limited funding. Under these circumstances medical researchers, too, became increasingly wary of what were seen to be the difficult value issues raised by intellectual approaches identified with the "social sciences". In the correspondence between members of the newly-appointed Health Services Research Panel of the MRC in early 1981 was to be found, for example, the expressed opinion of a prominent epidemiologist that "we must not ally ourselves with sociology and social administration -- this would be the death of our discipline."

The delicate position of epidemiology has been commented on by Kogan and Henkel in their comprehensive account of the workings of the Rothschild structure in DHSS-funded research. Unlike the civil servants quoted above, who found unemployment and health "untypical", Kogan and Henkel see problems in the collaboration between government and scientists as common or even routine. They point out that, at this time, epidemiology was:

moving from a position in which the nature of evidence and measurement and the choice of methodology were undisputed into one where, because social and psychological factors are ... assuming dominance, this is no longer the case. Moreover, this means ... that value issues are coming to the surface ... (p.20)

a fact which, in these authors' view, made funding of epidemiological research on the "customer-contractor" basis increasingly problematic for a major client, the DHSS. Accordingly, it should perhaps be no surprise that the DHSS handed back the 25 percent of MRC funds which had been transferred in 1973 for "health services research", removing them from the control of the social-medicine-dominated Chief Scientists Research Committee (see section 3.1).

It is consistent with this analysis that in the event, much of the major British research on unemployment and health was carried out in research units which were independent or semi-independent of government, but under increasing pressure, under governments of both complexions, to increase the "relevance" of the work they supported: – the MRC Unit for Epidemiological Studies in Psychiatry at Edinburgh University, the MRC/ESRC Social and Applied Psychology Unit (SAPU) at Sheffield University, the MRC- (and later charity-) funded British Regional Heart Study, and the Social Statistics Research Unit at City University, which received funds from the MRC, ESRC and Cancer Research Campaign (although until the Spring of 1985 half its Director's salary was paid by the OPCS). Mentions are made of "unemployment and health" in the Annual Reports of both the Department and the Council, as an example of the new-style "relevance" of Council-funded work to "social problems" (see DHSS 1983, p. 14, DHSS 1984, p. 12, DHSS 1985, p. 11.). The research carried out at Queen Mary College which criticised the time-series methods of Harvey Brenner was directly funded by the DHSS, and another important early riposte to Brenner was provided as a by-product of the DHSS's 1978 Cohort Study of the Unemployed. However, it should be noted also that the British study which had the greatest impact of all, in media terms, Fagin and Little's study of 22 of the 1978 Cohort families, was also given a small amount of support (£2,400) by the DHSS.

As the debate progressed, the sociologists and social and medical statisticians working in the dispersed research units tended to draw into a coalition, with each other and with "Manifesto" community medicine, to take up the ideas promoted by the Unit for the Study of Health Policy. This was the process of "enrolment" (see section 1.6) which will be traced in the chapters to follow. Economists COULD have become "enrolled" as sources of expert "knowledge claims" by those who sought to promote the idea that unemployment caused ill health. There was work produced by economists (Junankar 1986, Leigh 1987, Westcott 1987) which made this sort of claim. Also, other "intermediate bodies" which might be regarded as politically not dissimilar to the USHP (the Low Pay Unit, for example) employ economists as producers of appropriate knowledge. Therefore, the following account will also explore the reasons why economists who played the most important roles in the debate were to be found almost exclusively on one "side".

It will be argued that the major reason for this was that the unemployment



and health debate was, amongst other things, a field of contestation, not between *whole* disciplines, but between subdisciplines competing for a specific role. Because both groups dealt in numbers, it might have seemed that epidemiologically trained community physicians, and health economists “spoke the same language”. In fact the reverse was the case. At issue was *which* numerate discipline was successfully going to lay claim to the status of providing appropriate expertise to those planning and administering the health service, and those charged with justifying these policies in the eyes of the public.

It must be stressed that community medicine as a whole (the “establishment”, represented in the Faculty) laid no particular claim to intellectual hegemony over the discussion of health policy. The profession’s mainstream was content to rest upon the assurance of the medical closed shop -- by decree, no lay person was eligible to become a consultant in community medicine. By staking the subprofession’s claims on a “structural/environmental” model of the determinants of community health, the “Manifesto” group was taking a major risk. No such alarming potential resided in the work of the health economists. For example, Culyer suggests that:

Health care is of infinite value to none of us -- we smoke and drink and eat in ways that diminish our health and life expectancy ... All our daily behaviour denies that we value health infinitely (Culyer 1976, p. 5)

and Gravelle and Backhouse state firmly that: ^

The health of a population is determined by decisions taken by individuals in their capacities as voters, consumers, workers, employers, health service employees or government policy makers (Gravelle and Backhouse 1987)

If, instead of social and environmental conditions, the “determinants of health” were to be accepted as individual factors such as consumer behaviour, and inborn psychological and physical “fitness”, then there was (as Mackenzie, 1981 has suggested was the case in the early twentieth century public health debates) little intellectual space for any form of Community Medicine. Because economists can deal far better with individual consumer behaviour (which is, after all, part of their traditional business) and clinicians with genetic defects. As a result, Community Medicine was almost destined to cling to and seek to develop a public health approach, which stressed “the environment”, and in this

sense, the "Manifesto" group, whilst often in conflict with the mainstream of the sub-profession, must also be regarded as occupying its leading edge. And insofar as Community Medicine was the "customer" for so much of the activity of medical statisticians and medical sociologists, both as teachers and researchers, these changes in professional ideology drew the scientists' concerns along behind.

**CHAPTER 4**  
**PROFESSIONAL INNOVATIONS AND SOCIAL PROBLEMS**  
**1975–1982**

## **4.1 “Uncovering the social problem”**

Chapter 3 has described the institutional background to the unemployment and health debate, and outlined the processes of political and administrative change against which the events in this case study took place. This chapter will concentrate upon the ways in which members of professional and disciplinary subgroups sought to advance their (individual or group) claims to “expert” status by offering various ideas as “resources” in the attempt to form new alliances. Section 4.1 will look at the attempts made by some “marginal” members of the medical profession to use the unemployment and health debate to promote their own versions of a combination of clinical and social-scientific ideas as relevant to pressing social issues. Section 4.2 will describe the way in which the debate began to appear in learned journals, and to become seen as as “academic” debate, in which questions of method and technique took on prominence. Section 4.3 will describe three conferences on unemployment and health which provide examples of strategies of enrolment between academics, professional and activists concerned with Welsh and Scottish national issues.

### **4.1.1 An issue in health care**

In 1975 the Unit for the Study of Health Policy began to operate within Guy’s Hospital’s Department of Community Medicine, funded for five years by the King Edward Hospital Fund for London (“The King’s Fund”). USHP’s brief was “to promote informed discussion on health policy.” Its director was Dr. Peter Draper, who had previously been Senior Lecturer in Social Medicine in the days when academic departments dealing with public health were (as at Guy’s) entitled “Social Medicine”.

In December 1975, Draper read an article in the Financial Times, written by Lombard columnist C. Gordon Tether. It was about the work of an American sociologist M. Harvey Brenner. The topic was the effect of economic change

upon health, particularly the social and health costs of unemployment. The article struck a chord with Draper's existing interests in two areas (1) research on stress as a cause of illness, which he felt that the "epidemiology and community medicine establishment" were "carefully censoring", and (2) the question of the effect of the economic environment on health. These interests were strong enough for USHP to consider organising a conference on the topic, despite the fact that they saw unemployment as "Right off our main line" and "not one of the central issues we saw ourselves dealing with". Peter Draper remembered that at that time it "felt very lonely" to be tackling such an issue as "the link between the economy and public health." The legitimacy of the topic as treated by Brenner was harder to establish, because in his earlier work Brenner only dealt with mental health. As Draper put it

such data are notoriously soft, so one didn't feel on firm ground; had the early work been on stress and blood-pressure effects etc., I think it would have been assimilated -- at least by some of us -- much more easily.

Draper's colleague John Dennis explained that inflation rather than unemployment was seen as the major political and economic "problem" at that time, in wider political debate. However during the following year public concern about unemployment continued to increase as the figures continued to deteriorate. In August 1976, the number of unemployed persons in the UK was the highest total for that month since 1938: 1.5 million, or 6.4%. (Lancet editorial 22 January 1977 p 182). During this year, policy responses included several government employment schemes: the Work Experience Project, the United Vocational Programme, the Youth Employment Subsidy, and the Special Employment Needs Scheme. Draper and his colleagues decided to organise a workshop on the consequences of unemployment, which took place in January 1977. The workshop, entitled "Health, Unemployment and Ingenuity" was held in Abergavenny. USHP staff were gratified that a member of the editorial staff of the Lancet attended their conference. The Lancet editorial which followed was couched in sympathetic terms:

The direct health implications of being unemployed are many and obvious: Despite the various benefits that are payable, poverty and its concomitants – bad housing, poor nutrition, sickness, and social deprivation – remain disproportionately common amongst those who are out of work. (Lancet editorial, 22 Jan. 1977 p. 182)

Despite the marginality of unemployment and health to the concerns of USHP, they took up the task of promoting Brenner's work in the UK. Their ability to do so was greatly helped by the arrival of a medical student, Nick Joyner, doing an 'elective period' of study in community medicine, who wrote a high quality literature review on unemployment and health as his project. Joyner was one of the organisers of a series of meetings on "Issues in Health Care" aimed at students at Guy's, as was fellow student Stephen Wood. In November 1977, Prof. Peter Townsend was invited to speak to the "Issues in Health Care" group by Stephen Wood.

After the meeting, Wood wrote to Townsend:

It is all too infrequent that medical students have the opportunity to hear about such important topics as poverty and unemployment [although] ... I am optimistic (or perhaps naive) enough to hope that the ostrich-like attitude to social and economic questions so predominant in medicine may be on its way out.

He enclosed with the letter a copy of Joyner's project paper. As a result, Townsend referred Nick Joyner to his colleague at Essex University, Adrian Sinfield, who had carried out one of the few existing studies of unemployed people in Britain, for advice. However, it is the nature of student electives that, although they may inspire considerable effort and be of good quality, they are then "left behind" as the student faces his or her final year of study and examinations. Neither Wood nor Joyner (who both went on to study psychiatry) ever took up the issue of unemployment again, although their actions produced the first signals between the "medical" and "social policy" worlds that there was some medical interest in unemployment.

Another speaker at an "Issues in Health Care" group in 1977 was Dr Len Fagin, who dealt with the effect of unemployment on the mental and physical health of families. Like Wood, Joyner and several USHP staff, he was an occasional attender of meetings of a new discussion group, the Politics of Health Group (POHG) which began to meet during 1977. During this year, Fagin initiated a research project which was to have a major effect on the public debate. It was the one "psychological" study which received a great deal of media and parliamentary attention.

Fagin was born in Buenos Aires, where he also completed basic medical training.

That was what made me aware that there was an association between what went on inside the university and life outside.

he related. This awareness seemed to him to be lacking amongst doctors in England (he had not worked in Scotland or Wales). "In Latin America you are trained by circumstances to look at those sorts of concerns". He came to London to do further training and specialise in psychiatry. At this time, East London was, as he saw it an area where unemployment was rising faster than in other areas. Like some members of the British Regional Heart Study team (see section 5.2.1. below), Fagin knew of the work of Prof. George Brown on life events, and he, too, could see the potential for studying the effects of unemployment from this theoretical standpoint. He had also studied with Dr. Colin Murray Parkes, an acknowledged expert on bereavement and its effects. In bereavement, it seemed to Fagin, most individuals' "recuperative powers" asserted themselves after the initial period of acute mourning. In unemployment, however, individuals were not so well able to re-adjust. These were "clinical impressions" derived from his work with patients in East London, and he wished to test the impressions on a more representative group of families.

These ideas were combined with a concern with the connection between political and economic events and health. And I thought, there was little done on how unemployment affected families as opposed to individuals. The family was increasingly the focus of interest.

At first, Fagin and his colleague Martin Little, a psychiatric social worker, approached the Manpower Services Commission (MSC) to see whether they could study a factory closure. Objections from both management and unions proved too strong a barrier,

People just were not interested in talking to researchers in 1977. The unions thought we were on the management's side and the management thought we were on the unions' side.

They tried various other methods for picking up a sample of unemployed people -- standing outside benefit offices, going through GPs' lists and social services departments' referrals. None were successful. They then heard "through the grapevine" about the DHSS Cohort Study (see section 4.2.3. below). As Fagin put it

They were interested because they realised it was going to be a bit of a superficial statistical exercise, and it might be a useful adjunct to have a small descriptive survey attached to it.

The DHSS researchers agreed to contact a sub-sample of Cohort families to see if they were willing to be interviewed by Fagin and Little. "Access" to families now settled, they faced the problem of getting funding.

The MSC rejected our proposal. They said they didn't think they'd get a lot of return from a psychological study at this stage. Then we got turned down by the local health research group. They said I had not worked out my proposal enough

Eventually, after more "work on the proposal", it was submitted through the DHSS's Small Grant Scheme, and received £2,400 over two years. Like Steve Watkins of the Unemployment and Health Study Group, (see section 4.2.2.), Len Fagin was a registrar at this time, and, in common (increasingly) with registrars in many other medical subspecialties, they would have been not only permitted but expected to carry out research as part of their preparation for the post of consultant. Accordingly, Fagin used his study time to carry out the interviews.

The procedure for interviewing reflected the basic hypothesis of the study, that families might be differentiated into "good" and "poor copers", those who had overcome previous life events (crises) with differing amounts of success. This formulation demonstrates a certain degree of "moral fragmentation"<sup>18</sup> inherent in Fagin and Little's concepts, which appears to be present prior to the influence of DHSS officials upon their work. Or it may be that the formulation of the problem was itself the outcome of the lengthy negotiations between the researchers and other groups in their attempts to set up the study.

They interviewed 22 families chosen to be representative of the Cohort Study sample. The resulting report, which was a largely qualitative account of the families' experiences of unemployment, was then presented to their sponsors in the winter of 1981. The report and its reception by the media and in Parliament will be discussed further below.

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<sup>18</sup> The concept of moral fragmentation, which I have taken from Manning's discussion (Manning 1985 p. 21-22, see section 1.1), expresses the mixture of "individualisation" and "victim-blaming", frequently found in explanatory accounts of disadvantage or misfortune (including illness and mortality) which appeal to "selection" by personal characteristics such as "intelligence" and "coping ability".

#### 4.1.2 "The Reckoning"

In November 1978, Harvey Brenner was invited to speak at Guy's Hospital for the first time, at a meeting organised by the Unit for the Study of Health Policy. In the meantime, his work had also come to the attention of Granada Television's World in Action team. USHP staff did not remember whether this had happened through them or not. The programme was commissioned by Granada, who asked Brenner to study two areas felt to be contrasting in terms of unemployment levels at that time, Nottingham and Liverpool. Some USHP staff were left with the impression that the producer and the programme's researcher had suffered a certain amount of interference in their attempts to promote and realise the idea, that moves had been made to prevent it being shown, and that the fact that it was not billed in TV Times or the daily papers' TV guides was an attempt to minimise its impact. For example, on 12 Feb 1979 Peter Draper wrote to Brenner:

We ... half suspect a Machiavellian plot to minimise interest once the powers that be decide they couldn't actually KILL the programme (too many people knew of its existence).... I think it is most important that the Community Medicine establishment and the DHSS aren't allowed to get away with simply dismissing your work.

And a USHP staff member

couldn't decide what the truth of it was. I veered between thinking I was being either paranoid or naive.

However, a Granada journalist expressed shock at these ideas:

It is greatly to Granada's credit (he felt) that they do not interfere too much with how the programme is made ... What we make depends entirely on those 24 people [the World in Action team of researchers and producers]. If you work for the programme, you put up your ideas ... It's not just a question of having a bright idea, you have to fight for it. ... TV ... is an accident-prone process. There are no conspiracies involved in the making of documentary programmes ... Half our programmes are not billed in TV Times because no-one knows which one is going to be finished on time ...

The same journalist related on a separate occasion that Brenner had had to be filmed twice, at great expense to Granada:

It was the longest interview we've ever done on World in Action. It was so bad we had to pay for him to be flown back over to do it again ... Because the original 'rushes' were just too much like



Jerry Lewis playing the mad professor.

and a USHP worker admitted:

it had problems, it was what I call statistical television. The figures for deaths sort of sprang out of the screen at you.

In the event, however, the programme proved to be the most successful example of "information subsidy" in the whole of the debate, at least in the period 1978-1985. The day of the screening, alerted by Margaret Watts, USHP members attempted to overcome the absence of any billing for *The Reckoning* by phoning interested parties to tell them the programme was on, and also by taping it. Although unemployment was as yet at relatively low levels, (an annual average of 5.5% in 1978) the rate of increase under a Labour government, from 2.5% in 1974 (both these figures excluding school leavers), made it highly newsworthy. In deciding to use Brenner's work on unemployment and mortality as a vehicle to provoke debate on the role of the economy in relation to the public health, and thereby to suggest a more important political function for the new profession of community medicine, USHP had chosen well. (Figures from Baldock and Miller 1985 p. 140).

News of the programme's progress reached the attention of some Labour MPs even before it was screened. On 23 Jan 1 1979 Mr Geoff Rooker asked a Parliamentary Question to the Minister for Health:

Will my Rt Hon Friend institute an inquiry into the relationship between unemployment and illness, so that his Department is not stimulated to take that action following a forthcoming Granada Television programme?

To which Mr Albert Booth replied rather limply:

I see it as part of the role of my Department, working with the Health and Safety Executive, to watch carefully the relationship between unemployment and illness, and to take steps to institute good health and safety practices to avoid loss of employment in that way

thus genuinely or wilfully misunderstanding the point of the question.

*The Reckoning* was eventually screened on February 5th 1979. As A USHP worker related its impact:

The House of Commons requested a copy and then there was a lot of interest. USHP got phone calls from civil servants asking where they could get copies of the Senate report. We organised a small technical seminar which Harvey gave. John Fox came.

This was the first time I had met Fox.... The focus was on Harvey's model. People like Fox were concerned with the techniques. There were civil servants there too. One main reason for setting up the workshop was because of the interest shown by policy-makers. We kept getting phone calls from civil servants.

Present at the seminar were several people from the economic advisory staff of the DHSS. One of these explained their presence in the following way:

The DHSS were interested in knowing what the costs of the rising rate of unemployment were going to be as far as they were concerned. At this time the Labour party were in power and they were also worried about it. So for the time being the interests of officials and politicians were more or less the same. The second reason they were interested in whether what Brenner was saying had anything in it was because of health service planning needs. In trying to allocate resources between areas, you are always looking for indicators. If there was anything in this unemployment/health business, it might mean that either an increased demand might arise, or it might be useful as an indicator for allocation of services and local health authority provision. Did it mean we needed some sort of monitoring system?

The third thing was a Whitehall point. You see, departments are always looking for the best way of presenting their expenditure claims. At the time there was a lot of emphasis on Special Employment Measures and Job Creation ... So the question arose, to what extent should we be using the long term unemployed in jobs like unskilled work in the health service? Whenever a government department puts in expenditure claims, the Treasury resists them. Relating unemployment to special measures was a way of legitimating claims....The key issue as far as Brenner was concerned, for the Department, was, "here's all this song and dance - what work do WE need to commission in the light of it?" Both officials and Ministers wanted to know.

USHP, as "representatives" of the public-health faction within Community Medicine, had therefore succeeded in the first step of "interesting" government advisers and officials, even before the first academic publication of their ideas on unemployment and health.

#### **4.1.3 "Manifesto" Community Medicine and its political context**

In The Lancet of 17 February 1979 Peter Draper's team published the first British academic paper in the unemployment and health debate, entitled "Micro-processors, Macro-economic policy, and Public Health". The work of Brenner's which they quoted was his report "Estimating the Social Costs of

National Economic Policy", presented to the US Congress Joint Economic Committee in 1976.

The key finding [they wrote] was that a one percent increase in the unemployment rate in the United States sustained over a period of 6 years 'has been associated ... with increases of approximately 36887 total deaths.' (Lancet 1979, 17 Feb p 373)

They advocated, as a policy response "a transfer of funds from the social security side of the DHSS to the health side." These funds would be used to employ jobless people in health service jobs. Also significantly, they comment that:

As the number of "health economists" employed in Britain grows, ... it might be thought that many of them would be working on these issues -- the health implications of macro-economic policy . But ... nothing could be further from the truth. The overwhelming focus of attention is on ... how NHS resources are deployed and how they might be deployed "more efficiently" ... (p. 374)

and ask:

Is it not reasonable for community medicine, that part of medicine which is (or ought to be) specially concerned with the hostilities to health in the wider environment, to begin to work with HEALTH economists ... to develop a better understanding ...?...

This paper now tends to be remembered only for its discussion of "unemployment and health", It should in fact be seen as a vehicle for the wider ideas of what community medicine had to offer medicine as a whole, a concept of public health as a form of 'human ecology'. It must be placed in the context of the work (ongoing at this time) of a group of USHP workers and others in producing the document "Rethinking Community Medicine" which illustrates more fully the conceptual and practical background to their work on unemployment and health.

The usefulness of Brenner's ideas to USHP and to "Manifesto" community medicine lay in the concept of a "managed" economy, an essentially Keynesian idea, but with the addition that indicators of health should be used as well as purely economic indicators, as measures of an economy's ability to promote "welfare" as well as "wealth". Brenner and the USHP team shared a view that economic policies should be designed to take into account their contribution to human "health capital" and their health costs. This potentially offered an

important role to the public-health doctors, and one nearer to that of the MOsH of old. It also offered the possibility of an alliance with those social statisticians who had promoted "social indicators" in the 1960s and 1970s, whose role and very existence was to be threatened by administrative reviews carried out under the post-1979 Conservative administration.

During 1978 and 1979, these more general ideas began to have an organised expression in the form of Radical Community Medicine, which began as a ten-page newsletter and grew into a small journal. A series of informal group discussions at the annual conference of the Faculty of Community Medicine in June of 1978 led to a circular being sent round by a Specialist in Information and Planning from Liverpool health authority, Dr Alex Scott-Samuel. The letter proposed that a regular discussion group be formed. Those interested were divided fairly evenly between trainees (N=5) and academics (N=6), with only 3 fully qualified "service" SCMs being included. They met in late November, in the London flat of Dr Debbie Bartley, one of the co-authors of "Re-thinking Community Medicine", and drew up a "Manifesto" of their own, highlighting such issues as

- Can community physicians be apolitical?
- Are data value-free (a question which would tie them in with an existing group, the Radical Statistics Health Group.)
- Should health services be under local political control?
- Can we accept a cut-price National Health Service (at this time, Labour was still in power it should be remembered)

In January of 1979 there were 28 signatories (including Peter Draper) to a letter, entitled "Radical Community Medicine", which was sent to the Editor of the community physicians' professional journal, "Community Medicine", and duly published. In the same edition of the journal, an article by Scott-Samuel on "The politics of health" was also published, which must have added to the impact of the letter. Early in March, a meeting was held between some of the signatories (who had all been doctors) and non-medical members of the Radical Statistics Group (which included young statisticians trained in and committed to the ideas around "social indicators"). This group proposed the publication of "a ten-page newsletter, say quarterly." Scott-Samuel took on the role of "catalyst" (as he saw it) or co-ordinator, on a temporary basis, a role which was to continue for five years.

Shortly after the screening of "The Reckoning", a Labour MP, Jeff Rooker,

wrote to the Undersecretary of State for Labour, John Golding expressing disquiet at the Labour Party's position on possible health risks of unemployment. The answer to this letter indicates that there may be a risk (both for participants in a debate and for observers) of exaggerating the effect of having conservative rather than social democratic parties in power upon the reception of politically sensitive research. On 1 March Golding replied:

Although there is an association between unemployment and ill-health, it is difficult to prove that unemployment is itself a cause ... since unemployment is often a consequence of poor health, accidents or prolonged illness [here a note scribbled on the margin of the letter exclaims "with 1.4 million!"]. Some researchers have deduced, from much the same statistics as Professor Brenner, that employment, rather than unemployment, is the most important determinant of ill-health and mortality. There are also problems in proving that unemployment rather than poverty causes ill-health, since the factors are all interrelated. These questions are being explored in the DHSS's research programme, and to some extent also in the Department of Employment's research on the social costs of unemployment.

The letter will have been written by a civil servant as a "Private Office case", and demonstrates a considerable awareness of the research. The reference to the dangers of work could be a reference to the deliberations of the Black Report group. Or it could be to the 1977 debate between Brenner and the leading American antagonist of his work, Joseph Eyer (who could be considered politically far to the left of Brenner). As is often the case, "backstage" civil servants are intellectually on top of the issues raised by MPs, and in a position to outbid left-wing pressure whether from outside the governing party or within it.

On March 31 1979 a leading article "Does Unemployment Kill?" was published in the Lancet. It reminds readers that unemployment has now reached what its editorial staff regarded as an unprecedented postwar level of 5%, and comments:

If [Brenner's] theory is correct then the rising unemployment rate in Britain ought by now to have been associated with mortality considerable enough to put the nation's economy firmly in the domain of community health.

This leader echoes the statistic that had been found so "media-presentable" by World in Action, that "a 1% rise in unemployment sustained over a six year period could bring about 36890 deaths in the United States," and adds:

If unemployment is indeed the new 'great plague', then perhaps we should call up the shades of Chadwick and his peers and make a public health attack with all the missionary zeal and authoritarianism of the nineteenth century.

In these words, the leader writer expresses precisely the relevance of Brenner's work to the ambitions of the new subprofession of Community Medicine.

In May, a General Election swept a new Conservative government into power with a large majority. This did produce some change in the ways in which certain participants dealt with the debate on unemployment and health -- participants who were actively identified with the Labour party no longer had to take care not to "embarrass ministers" in their own Party <sup>19</sup>. The earliest discussions of the relative merits of Brenner's and Joseph Eyer's approaches had taken place at least in part in the London-based Politics of Health Group, whose members at that time ranged politically leftwards from the left of the Labour Party. Once Mrs Thatcher came to power, it began to be seen as an advantage if research on unemployment and health produced "something to beat the government over the head with", in John Dennis's words. But, on the other hand, the restrictions on civil service "expert" (and other) recruitment, and on the funding of universities, meant that members of "expert" disciplines had to be both more careful and more entrepreneurial in the contests for valued roles and resources. Certain sorts of value claims, which might have embarrassed a social-democratic administration into action (or at least into "research") would increasingly be found to fall upon deaf ears.

The Unit for the Study of Health Policy suffered a setback at this point. Its Kings Fund money came to an end, and it was forced to fall back on "a temporary Band-Aid from the Charities Aid Foundation". The group did not publish any further papers in academic journals on unemployment and health. According to Unit staff, unemployment and health had never been a central project. One felt that

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<sup>19</sup> To take three members of either the "Rethinking Community Medicine" author group and/or the group around Radical Community Medicine, John Dennis of USHP was active within the Labour Party as an adviser on health and related issues, Jennie Popay, who worked at USHP during the making of *The Reckoning*, and Alex Scott-Samuel became members of a "Front Bench Advisory Group" on health, convened by Michael Meacher, the Shadow Secretary of State for Health and Social Security, in the summer of 1984. Later, they both became members of the official health subcommittee of the Labour Party National Executive which drew up the health proposals for the 1987 election.

After [the 1979 paper] came out ... we had played our role as a policy group, It was on the agenda now.

It was not so much the priority given the issue by USHP, but the expertise of Peter Draper's team in producing information subsidies on health topics, which made them the "entrepreneurs" of this social problem.

Peter Draper remembered that

We felt very lonely suggesting links between the economy and public health in those days; the "causal" experts, the epidemiologists, seemed to be silent on such matters ... Nowadays [1987] .. it all seems so obvious, but it felt quite different then! Unemployment and health felt as though it was something of a research indulgence rather than basic territory.

## **4.2 Academic debate begins**

### **4.2.1 Economy and mortality**

Brenner's own paper "Mortality and the National Economy – A review, and the experience of England and Wales, 1936–1976" was published in The Lancet on 15 September 1979. Draper and his colleagues remembered having "pressed" Brenner to write the paper, and that he had been "always reluctant to commit anything to paper".

In the light of the role Brenner's ideas were to play in subsequent events and argument, it is interesting to re-examine some of what he actually said in this first publication in a British academic journal. His approach is Keynesian (as opposed to the Marxist approach of Eyer), exemplified in the following:

Long-term economic growth will also moderate the problems associated with economic instability: management of the national economy improves, health care gets better in quality and availability, and more substantial income support can be provided for displaced workers and other non-participants in the labour force. (p. 573)

There is an underlying theme in his work, not just in this paper, that economic management can and should be used to moderate health consequences of inevitable technological change.

The reason Brenner gives for the long and diverse "lag periods" in his

model are both medical and sociological. Not only are today's "killer diseases" chronic ones such as coronary heart disease, stroke and cancer (in contrast to the 19th century plagues of acute infections), but Brenner is quite clear that recession's health-related effects upon those still IN the workforce include the effects of work stress, downgrading and re-integration, as well as joblessness:

In this sequence of 'downward social mobility' the illness process begins with recession, and, within two or three years, the likelihood of mortality is greatly increased ... For the downwardly mobile, the next major source of stress usually occurs during their reintegration into the economy. (p.568)

Another point raised in this paper and often overlooked when it is quoted, is that Brenner puts forward, in his penultimate paragraph, the idea that "these instabilities in economic growth also account for the socio-economic differentials in mortality", and "The mechanism by which unemployment and rapid economic growth act to slow the secular trend of mortality-rate decline is through a widening of the socio-economic differentials in mortality." This was to provide a link at a later stage between the unemployment and health debate, and the debate on health inequalities. Very little of this complexity found its way into the public debate, either in media accounts of 'Death on the Dole', or in Parliamentary Questions, although, in hindsight, many participants regarded the unemployment and health debate as "just part of the debate on health inequalities"<sup>20</sup>.

Proponents of "manifesto" community medicine were never particularly interested in the validity of Brenner's methods. They had been trained in the idea that early successful public health reform had been carried out on the basis of theories of contagion now held to be totally erroneous. This approach was frowned upon by economists. One remarked:

The people who would have liked to make capital out of Brenner's work basically couldn't understand it. So they went to other people who could understand it and were warned off it by them.

And another derided the "soap box methods" used (as he saw it) by Brenner. Neither of these economists were surprised by the impact of Brenner's work on

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<sup>20</sup> An attempt to place these considerations more centrally in the early debate failed, as reported in section 4.3.2 below.



policy debate, however. Strongly expressed claims, they felt, were what attracted the attention of the media and policymakers, not academic rigour. What they deplored was the role played by an academic in direct public promotion of his own ideas.

#### **4.2.2 The issue community takes shape**

There was a relatively low level of "academic" activity around the issue of unemployment and health during 1980. One reason for this may be that this was the year in which the "Black Report" appeared, and attracted the attentions and energies of the groups involved in health policy debate, although no explicit connection was made between the two issues at this stage.

In other pieces of backstage activity, the shape of pressure-group participation in the issue community was also being determined. By January of 1980, Nicholas Hinton, Director of the National Council for Voluntary Organisations, had drawn up a proposal for a "pilot study", to replicate Harvey Brenner's research in Britain. It was envisaged that the National Council for Social Service would employ 2 or 3 people to collate the necessary statistics, which would then be sent to Baltimore for analysis by Brenner's computer model<sup>21</sup>. It also proposed that a Steering Committee be set up including representatives from NCSS and "selected UK institutions". A budget of £26,000 was estimated.

One participant in the events at this time explained this development in the following way:

Nicholas was trying to make his mark in political circles and saw unemployment and health as an issue. NCVO was not very influential before he joined it. He came in as a young bright whizz kid and wanted to make his mark, frankly. [Projects Department of the NCVO] was the department in which major growth was prized, because they could get small bits of money from the government to fund small pieces of work. Unemployment and health was one of the bright ideas... It was a new idea, and they wanted to expand the Projects Division, it was seen as a way of getting money into the organisation ...

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<sup>21</sup> The proposal not to analyse the data in Britain is, of course, an indication of the extent to which Brenner's "model" had become a "black box", to British researchers, as discussed in section 2.1. above and section 4.3.2. below.

Have you seen the buildings the NCVO have in Bedford Square?  
Well, you can imagine the rent...

In this account, an entrepreneurial process involving organisational and personal "cycles of credibility" is offered as an explanation of the way in which workers in VOLUNTARY organisations entered the unemployment and health debate. The similarity with which the notion of "Having an bright idea and fighting for it" is dealt with by participants from such diverse backgrounds as the media, the academic sphere and the voluntary sector illustrates the common features of claims-making activity throughout the issue community.

Also in early 1980, a new organisation was formed which was to have some influence upon the course of the debate. Jennie Popay had by now left USHP for a post at the Study Commission on the Family. She and Trevor Davison, a Project Officer at the NCVO, shared a long-standing interest in unemployment, first developed by their experiences as students. At the beginning of 1980, Davison and Popay began to promote the idea of a Social Costs of Unemployment Forum, with the support of the NCVO, "to collect and disseminate information to ... the increasing number of organisations and individuals at national and local levels [who] were acutely aware of the potential impact of unemployment but were unsure as to how to tackle the problem." (NCVO Minutes, DG (81) 80). Beginning with 8 members, SCUF grew swiftly. By April it had 30 members, including several members of the Radical Statistics Health Group, and community physicians of "Manifesto" tendencies, and was beginning to be inundated with requests for information and speakers.

In the Spring of 1980, Brenner renewed his contact with Liverpool by returning to address an EEC conference on "urban social problems". This time the invitation came from Alex Scott-Samuel who "was anxious to expose the delegates to more than the comfortable lounge of a cushy hotel," as he told journalist Steve Connor of General Practitioner (7 March 1980, p 37) In June, the Liverpool Daily Post produced an exemplary piece of medical journalism on Brenner's work. It began with the "human interest", a tragic account of an individual's suicide, headlined "Death of a Good and Willing Worker". Beneath was a series of clearly presented graphs adapted from The Reckoning. Granada, wrote David Utting, had brought Dr Brenner to Merseyside to see the human reality behind the statistics. Utting reported that Brenner had both pledged his "active backing" to the NCVO proposal and expressed interest in Scott-Samuel's plan for an "intensive study in a small area badly affected by

unemployment.”

This latter point will have been a reference to the founding of another small group which was to be more persistent in its influence on the progress of the debate. This was the Unemployment and Health Study Group (UHSG), based in the recession-stricken North West of England. They held their inaugural meeting, organised by Scott-Samuel, on 19 June. Their initial discussion centered around various projects proposed by different members of the group. They also discussed the research proposal from the NCVO. At their second meeting in September 1980, an M.Sc. student in health economics from York University, Pat Kennan, spoke to the UHSG about the results of one of the DHSS-sponsored research projects which had been mentioned in Parliamentary Answers. This was her own Masters project, an attempt to replicate Brenner's econometric analysis on British statistics, supervised by Hugh Gravelle at Queen Mary College, London. This was eventually to form the basis of a paper which, a year later, spearheaded the attack on Brenner's claims.

This was, therefore, the year in which pressure-group activity took shape. A considerable number of research proposals were also drawn up during this period, none of which ever materialised into published work with impact on the debate. However, links began to be forged which were to persist and to influence not only the unemployment and health controversy, but the future of “Manifesto” community medicine, the activity of research teams, and Labour party health policy. In tracing out the history of these links, we will begin to see the extent to which the content of the knowledge claims presented in the debate was constructed by the wider strategies of the groups involved. “Unemployment and health” was a vehicle for diverse groups' and individuals' projects, as well as a topic in its own right.

In July 1980 the debate re-appeared in the Commons when Dennis Skinner asked Patrick Jenkin “if he will establish an urgent inquiry into the link between unemployment and heart diseases, mental stress, suicides and other health problems with particular reference to those areas in the country where unemployment ... is of a very high and long lasting nature.”

In reply Jenkin assured Skinner that his department

is already involved in studies of health and the unemployed, including questions of their interrelation. We are also co-operating with outside researchers on a study of long-term

unemployment and mortality... We are aware of the research being done by Prof Brenner and by the department of applied economics at Cambridge, ... There is as yet no published work which establishes with any certainty a causative link between unemployment and ill health."

The "outside research" referred to here was that being carried out by Hugh Gravelle of Queen Mary College, London, and his M. Sc. student. Another contribution to the debate was also in the process of being produced at this time within the DHSS. There was a longitudinal study of unemployed men (the DHSS Cohort Study) being conducted in the department which, in 1980, carried out its third "sweep". In the questionnaire, an item on health was included, which aimed to see whether those men who had remained unemployed for the full duration of the study had experienced worse health than those who found work again. These were both relatively inexpensive pieces of work, which made full use of the skills and motivations of a subdiscipline, economics<sup>22</sup>, at least some of whose practitioners had a self-conscious "interest" in demonstrating to client groups the ability of their methods to deal with questions of health policy.

#### **4.2.3 "A pilot study"**

However, neither of these new studies had reported by December of 1980, when Len Fagin and Martin Little (see section 4.2.3.) submitted their findings to the DHSS, the sponsors of their study. It seemed to them a somewhat alarming experience, as they remembered it:

We met a sort of committee in a very large room with one of those huge round tables, like a NATO meeting, with a huge space between "us" and "them" on the other side.

"They" were "about twenty DHSS dignitaries" who

used a lot of words like pilot, descriptive, sampling. Methodological points.

Fagin observed that in the public debate which followed,

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<sup>22</sup> Although Cohort Study researchers were drawn from a number of social science disciplines, the authors of the first paper to make use of the new "health" questions were both Economic Advisers.

George Young and Gerard Vaughan ... used those exact comments, the very words that were used that day.

This insight into the strategic dimension of official "forms of words" was later confirmed to me by a senior government research manager, who considered it to be of the essence of the relationship between researcher and political administrator. In answer to a question about how researchers INSIDE government maintained the boundary between "fact" and "opinion", she remarked that it was

a very fine line

which was maintained

in practice, by LANGUAGE. We draw the line by the use of very careful, heavy, official prose, an attempt to make things as factual and flat as possible. ... [These forms of words] carry a lot of tacit meaning. Once they have been agreed, for example, for use in an answer to a Parliamentary Question, they tend to be used over and over again.

The emphasis on the "descriptive" nature of Fagin and Little's study had in fact been negotiated explicitly, in early 1979, between the researchers and the Central Statistical Office, whose Survey Control Unit has to give "recognition" to any survey carried out by government departments. There was a conflict between two aims shared by the researchers and the Control Unit: On the one hand, people responsible for the main Cohort Study did not want too many families to be interviewed by Fagin and Little (who no doubt had their own constraints of time and energy as well), in case this influenced the responses to the next sweep of the whole sample. On the other hand, the Control Unit had to urge the researchers to interview sufficient numbers of families to allow statistically meaningful comparisons to be made between, for example, those unemployed men whose wives were employed and those whose wives had no job. It was in response to this dilemma that Fagin himself had emphasised the "descriptive" and "pilot" aspects of his study.

There is more or less total disagreement between the researchers and the officials over what happened next to Fagin and Little's report. Fagin himself typed the report, so that 200 copies would be available for public consumption. These were issued by the DHSS -- for example, the Regional Heart Study team wrote off for a copy to the Elephant and Castle, and received one without delay. But Fagin felt that some attempt had been made to minimise the impact of the report. If this is true, it was a failed attempt. There is no example in the

unemployment and health debate of a (real or suspected) attempt at "government suppression of information" which failed to promote greater media interest than would have been aroused in its absence. Pressure groups (and, increasingly, academics) know this, so that there is some temptation to imply a "cover-up" in order to gain the attention of journalists (see Chapter 7.3). Officials who spoke frankly about departmental activity in the unemployment and health debate were insistent that there was no attempt to limit the availability of the report.

On 9 June 1981, Sir George Young made a statement in the House of Commons in reply to several questions from Labour MPs about Fagin and Little's work. The researchers were not happy with the statement. Fagin took Young to task for failing sufficiently to emphasise that although the health of two men had admittedly improved after they became unemployed, a deterioration in health was found in more families. At the same time, he asked that the DHSS lower the price of the report (£6.00) and make more copies available. Young gave a long and considered reply, stressing that in his statement he had admitted that "it seemed reasonable to assume that there is some association between unemployment and health" and that he had been "simply pointing out the difficulty of drawing clear conclusions about cause and effect" from a small study.

Shortly afterwards, Fagin also had to reproach the editor of the News of the World for what he felt had been a "sensationalised" report of the study's findings, published on August 16. The argument died down during the summer parliamentary recess, but re-emerged in October when a group of Labour MPs, organised by the Junior Opposition Spokesman on health, asked a battery of oral questions on unemployment and health. Dr Gerard Vaughan, then Minister for Health, had the task of replying. Like Young's, Vaughan's replies angered Fagin, who felt that the Minister had "misguided Parliament ... for what I can only conceive are political purposes".

Even so, it was not the accuracy of Vaughan's statement that Fagin quarrelled with, but rather its emphasis. Rather than taking the acknowledged preliminary nature of the research as an indication that more and bigger studies were needed, the fact that it had been "only a pilot" was being used to minimise the importance of the findings. In his reply to Fagin, Vaughan spoke of the research at Queen Mary College (Gravelle's work with Pat Kennan) and

the Cohort Study as responses to the need for more knowledge, and made rather vague references to "fresh work designed to clarify the nature and significance of any unemployment-health links ... with the benefit of expert advice from independent sources". This correspondence between Fagin and Vaughan was placed in the House of Commons Library in mid-November, and, after another small flurry of letters between Fagin, the Labour MPs who asked the original questions, and Vaughan, the matter rested there. By this time, also, major developments had taken place in the academic debate, which will be discussed below.

Fagin himself, three years later, felt that the major impact of his and Little's work had been on other professional groups, rather than on policy-makers. Another effect he felt was that, by 1984, the media tended far less to treat the unemployed as "scroungers", and more to admit that unemployment could not be the fault of individuals. This had also been part of the purpose of the Cohort Study as a whole, to investigate the question of the "replacement ratio" in order to see whether a large proportion of the unemployed were indeed "better off on the dole", as some headlines in the late 1970's had described them. In the longer term, therefore, the impact of the smaller study (as perceived by Fagin) and the larger one of which it was a part were, in this sense at least, consistent and in the desired direction.

### **4.3 The "Celtic fringe" conferences**

#### **4.3.1 The Cardiff Conference**

In 1981, two pressure groups adopted the unemployment and health issue, without whose promotion it is doubtful whether the work of Brenner would have continued to have quite the same amount of impact. These were the Welsh and Scottish National Parties, which, for various reasons (Drucker and Brown 1980, Clarke and Drucker 1978) were in a relatively active phase of their own development at this time. The idea that unemployment could harm health was particularly striking to them because of the way in which unemployment is regionally distributed in Britain.

The conference which had an impact on the subsequent development of the

academic debate was that sponsored by the Welsh National Party. It was held on April 10 1981 in Cardiff, and speakers included a mixture of political medical and social-science "names". Brenner duly put in a well-publicised appearance. The conference was attended by a wide range of experts from all over Britain, as well as local activists. Speakers included such respectable figures as Prof George Brown (co-author of "Social Origins of Depression", and the leading British authority on life event research), and prominent members of the local medical community, as well as a consultant psychiatrist associated with the Welsh National Party, and a research officer for the Wales TUC. In the words of its organiser, Senior Lecturer in Community Medicine Dr Stephen Farrow, his objective for the conference was "to put Social Medicine back on the agenda of my own Department." The unanticipated furore which it caused, including an American camera team, was not, for that reason, unwelcome although it may not have been strictly intended. Although the Cardiff conference was planned as a claims-making exercise, aimed at influencing both the theory and practice of community medicine, and wider public opinion on government policy, the most lasting result of what took place was a contribution to the moral and technical fragmentation of the issue.

The Lancet wrote of the conference (April 18 1981):

The causal chain – from economic depression through organic illness to premature death is instinctively believable ... [but] over the teacups there were serious criticisms of Brenner's findings: UK workers do not seem to be able to confirm his work, but this criticism is not yet, as the Americans would say, in the public domain

One of the events that took place over the teacups was an encounter between a deputy editor of The Lancet and an economist whose career had included spells working as a government adviser (on the Cohort Study amongst other projects). As he remembered:

I went down there [to Cardiff] and watched the NBC cameras and all the carry-on. [two colleagues] were there too. We sat ourselves in the first two or three rows. Early on, I threw in a couple of Cohort results we had obtained. Brenner said the Cohort Study results missed the point as most of the effects of unemployment were not on the unemployed themselves but on the health of the non-unemployed. Well, this I could not understand ... It was just his way of dismissing the Cohort Study results.

After tea, a man came up to me and said he liked my question ... he turned out to be the deputy editor of The Lancet. I asked if he



would welcome a paper on this.

The suggestion by the editor of the Lancet that a high-status medical journal might “welcome” papers by economists on unemployment and health was an important step in the shaping of the next stage of the “technical” debate, which will be discussed in section 5.1.

#### **4.3.2 The Glasgow conference**

During 1979–1981, because of USHP’s funding problems, Peter Draper was having fairly intensive discussions with the members of his Steering Committee, and other senior advisers. This group included several prominent characters in the Scottish health policy field: Sir John Brotherston, ex-Chief Medical Officer for Scotland and Emeritus Professor of Community Medicine at Edinburgh University, Dr. John Loraine, an eminent endocrinologist in semi-retirement who headed Edinburgh’s Centre for Human Ecology, Dr David Player, Director of the Scottish Health Education Group since 1975, and Mr T. Drummond Hunter of the Scottish Health Services Planning Group. Some of these people also had sympathies with the ideals of Scottish nationalism, more specifically with the notion that there was something special about the Scottish approach to Community Medicine, as reflected in the Gilloran Report (Joint Working Party 1973). By early 1981 (before Cardiff), there had been links established between Peter Draper, David Player, Sir John, a senior medical officer in the Scottish Home and Health Department, and Iain More, a full-time worker for the Scottish National Party.

The connection between USHP thinking and that of a certain group of Scottish health planners and policy advisers was centered around a set of ideas less directly related to devolution, perhaps exemplified by Drummond Hunter’s 1976 paper on The Reorganised Health Service in Clarke and Drucker’s book “Our Changing Scotland”:

In Scotland [he wrote] a more imaginative stance [on the 1974 health service reorganisation], which may have reflected oft-repeated views of the CMO, Sir John Brotherston, was adopted ... reorganisation in Scotland was not simply an attempt on the part of government to regain control of a runaway health service ... (p. 31) Health ... has less to do with services than with life-chances and life-styles ... (p. 35) ...

It was the uniqueness of the Unit for the Study of Health Policy that it briefly

attempted to combine all these issues in its work. The ideas came together into the Glasgow conference scheme because of largely informal links between the Scottish participants, a fact perhaps reflected in the constant good-natured references to "a Mafia".

For the senior medical officer, it had been his political ideas and contacts which lay behind his interest in the issue. "We [self, Player and Brotherston] are a sort of Mafia."

The SNP [had] taken a political and psychological knock [by the combined effects of the failure of the devolution vote in 1979 and the return of a right wing government] ... we were using unemployment and health as a political issue. In fact, I now think this was wrong. It should have been used not as a party issue, but as an evil in itself.

For Iain More:

[David] Player thought unemployment and health was a good issue ... There were 3 people in the SHHD who had done some sort of studies on unemployment and health and [one of them] is on the policy committee of the SNP. We thought that a quality conference with a minimum of party politics would be a good thing, at least that was my first idea. We [SNP] would then get the credit for it. ...Brenner appeared to be the most outstanding person in the field. So I wrote to him. He was interested. He said he'd be across in the UK for that conference in Cardiff ... We [SNP] try to do one thing like this every year. ... You need to build up credibility in the eyes of opinion makers. Short term things are most important to political groups like the SNP ... You get your mileage out of something like this and then lose interest.

It is interesting to note here More's account of what constituted the "cycle of credibility" in a political group, and his awareness that the form taken by this cycle leads to an emphasis on the short-term. Like the policy advisers studied by Prince, and (despite what he seemed to think, in many ways) the academics, More was caught in a conflict between "intellectual fascination" and commitment to finding short-term problems to which SNP policies could be offered as plausible answers,, one of the organisational exigencies of his professional role.

For David Player, according to one colleague:

His own background was poor - so he was conscious of

Scotland's health problems ... He was generally concerned. He is a close friend of Peter Draper's and Gerry Morris's ... They were also in touch with Raymond Illsley. They are a bit of a Glasgow Mafia really ...

So there appear to have been institutional (through the Steering Committee of USHP), philosophical, political (through the SNP and wider group of nationalist sympathisers), and informal links between the leading figures behind the Glasgow conference (they were "golfing buddies" according to one participant).

As well as being seen as a "good political issue", unemployment and health articulated several concerns common to community medicine, health education and "promotion", health economics and NHS planning . This is the source of the impact of Brenner's ideas in these professional circles, despite his methodological vulnerability and awkwardness in putting his message across. However, several participants in the organisation of the Glasgow conference later expressed a view that using Brenner's work too uncritically and too "politically" had perhaps been a mistake.

The two academics brought in to debate directly with Brenner were Steve Engelman, Senior Lecturer in Health Economics at Edinburgh Medical School's department of community medicine, and Mike Porter, the medical school's sociology lecturer, also an economist by training. Like Porter and the USHP staff, Engelman had been finding Brenner's work useful in teaching, since the late 1970s. Engelman's memory was that Brenner's work was not, at this time, thought to be particularly controversial, but he had thought it "interesting", and the sort of thing medical students ought to know about. From the technical point of view, like other economists, Steve Engelman realised Brenner's work was "econometrically unsatisfactory" but that it had "potential shock-horror value".

Usually, econometric work has problems, but other people take it up and work on it and either it improves or it gets forgotten.

The way in which academics involved in the Scottish debate turned away from Brenner provides an illustration of what can happen when a knowledge claim is pushed too far too soon (see section 2.1), exposing its advocates to "dissidence and betrayal". The actor-network which used Brenner's work to impress doctors and medical students was strong enough to defend its status as an unexamined "black box" as long as it was (in Engelman's words) "not very

controversial". By the time of the Glasgow conference, however, the paper on the Cohort Study (Ramsden and Smee 1981) and Gravelle, Hutchinson and Stern's critique of Brenner's method had been published, with great impact (more fully discussed in section 5.1). Having attracted hostile attention by producing political controversy, Brenner's model was to prove too weak an ally, his own methods could be turned against the arguments of his supporters. The evening before the SNP-organised conference, Brenner spoke to a small invited audience at a seminar held in the University to "discuss the scientific issues". Participants remember that at this meeting Stern and Gravelle made an intensive and successful attack upon Brenner's position. Because the underlying concern of even his *supporters* was not "the effect of unemployment on health", but the relationships of their subdisciplines to the powerful forces represented by the medical profession, Brenner's early academic advocates readily relinquished his ideas once they came under this type of fire. Most of them also abandoned "unemployment and health" as a major work priority.

In the short term, however, the outcome of the Glasgow conference satisfied at least some of the organisers. Like the USHP, the SNP succeeded in presenting Brenner's work as a "information subsidy" – the press and TV took the bait. However, as a non-party, academic conference aimed at serious consideration of evidence on a public health issue, the outcome was less favourable. Fragmentation had by this time progressed too far for the "truth" of Brenner's claims even to be discussed in a "legitimate" way at such a non-technical type of gathering.

David Player thought the Glasgow conference had been a success, as did one SHHD official who felt it had had "an enormous impact", although he was less happy about Brenner's presentation. However, Mike Porter described Brenner's speech as a "disaster", and in the view of a Glasgow University health economist:

You'd think someone would send Brenner to 'Subject A' [classes held in the US for academics to teach style and presentation technique] to teach him to communicate. It is atrocious really, a Fourier analysis in fifteen different slides.

A scathing account was given by an Edinburgh medical school academic, which provides an example of what a scientific "black box" begins to look like when informed opinion no longer accepts it as such:

It was terrible to be preached at like that for so long. ... it was like an evangelical meeting, no one had a chance to talk back Oh yes, they gave Harvey Brenner a chance to talk back and we got MORE figures and charts and he unfolded all this other data. He tried to blind people with figures any time anyone tried to make a point ... It wasn't fair.

Instead of being seen as technical virtuosity, Brenner's complex methods and large volume of data (some of which may have been added in to the presentation as a response to Gravelle and Stern's attack of the previous evening) are now referred to as "preaching" and "BLINDING" people with data.

Iain More admitted to having faced some criticism for his use of Brenner as centre piece:

It was suggested to me that by using Brenner we could destroy the issue ... The argument was that people interested in the politics were not really interested in serious research. You get you mileage out of something like this and then lose interest. This is true to some extent.

At the end of the conference, Richard Smith, an Assistant Editor at the BMJ and a graduate of Edinburgh University's medical school, highlighted the non-academic agenda of the conference for his readers by reminding them that Sir John Brotherston had called for "a Scottish forum where issues like this one that are especially important for Scotland could be debated." Accordingly, academic participants in the conference were not optimistic about the possibilities for "further research", despite the frequent mentions of research in answers to Parliamentary Questions at this time. Steve Engelman and a colleague at Glasgow University, John Forbes, put in a proposal subsequently to the Scottish Health Services Research Committee<sup>23</sup>, although they were aware that this topic was, as Engelman put it "not health SERVICES research ... but I thought it was of considerable public interest." The £120,000-130,000 they had asked for was, Engelman realised, a lot of money, but the only appropriate method was an expensive longitudinal design "...if you're really going to throw any light on the matter." Despite having lowered their original financial estimate, their application was not successful (but for an account of the fate of Forbes and Engelman's proposal from within Whitehall, see section

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<sup>23</sup> a body whose raison d'être was similar to that of the CSRC and MRC Health Services Research Panel discussed in section 3.1.

John Forbes saw the future for research in the area quite clearly:

We tried [himself and Engelman] to get a proposal together [in 1982]. The Scottish Office said they didn't have the money, they sent it to the Dept of Employment, the DHSS, all around the place. It was just after the [1983] General Election that they wrote back saying it had been given low priority ... Now, I feel it is curious the way they turned it down. They might have jacked it on methodological grounds or said it was not do-able. One way you might get them to support your research might be to say you believed there were econometric problems with any existing piece of work which seemed to show an effect. This can always be done: 'Dear Minister, We think there are problems with Brenner ...' But as soon as you get a rejection like this one, you lose interest. It is a big investment that has gone to nothing.

These comments show how participants viewed the process by which research can be presented so as to have an impact both in terms of policy debates and in generating future research. It shows how researchers, activists and professionals puzzle over which alliances are the best ones to seek, which groups need to be "inter-ested" and what is the best way of accomplishing that aim. The difficulty of choosing the best strategy is further illustrated in the fate of the next Scottish conference on unemployment and health.

#### **4.3.3 The Stirling "Consultation"**

The Edinburgh Medical Group Consultation "Work, Health and High Unemployment", which took place on 1 - 2 June 1982, had been in planning stages almost since the beginning of the Glasgow planning process. Unlike Glasgow, which had been put together by Iain More and a member of the SHEG staff, Stirling had a Steering Group, which held its first meeting on 23 April 1981. The purpose of the Stirling "consultation", as it was termed, was "to be complementary to Glasgow rather than competing." (Steering Group minutes 23/4/81). It was seen as a more overtly "political" event, in that political subjects would be explicitly raised, instead of being implied by the factual claims presented as "research". And yet, in its organisation it was far LESS "political" - no particular party or pressure group was involved - Iain More was invited to give advice only. A colleague of Player's at SHEG related:

... Stirling was an attempt to provide a responsible medical basis

for the unemployment and health debate ... Dr Player saw that there was another dimension to the debate, an economic and political as well as an epidemiological argument....

Although David Player and others regarded Stirling as "less political" than Glasgow, the Scottish Office were not convinced. In May 1982, a letter arrived at SHEG from St Andrews House which criticised Player for becoming involved in the Stirling conference. The letter's author (a civil servant, not a politician) wrote:

I know you will not take it amiss if I offer my strong personal view that it is stretching the Group's role well beyond the limits that the Management Committee are likely to endorse ... What is the health education point? If unemployment is bad for health, are health educators to argue for higher levels of employment than the combined wisdom of economists, industrialists, and politicians would otherwise achieve? If it should transpire that unemployment is sometimes good for health, is SHEG to be in the business of pressing for Government health warnings on appropriate pay packets?

The letter's author in fact puts his finger squarely on the point that Peter Draper and his USHP co-workers, the "Re-Thinking Community Medicine" team, the group around Radical Community Medicine and the Unemployment and Health Study Group had been trying to make since 1979 by using Brenner's work on health and the economy. That is, the "Manifesto" faction's claim that a true "community medicine" should be involved in wider issues of policy and politics. There was disagreement, however, among the conference's organising group, over what *sort* of alliance between "medicine", "science" and "social policy" should be pursued. Some organisers and speakers felt that the major theme of the conference should be "inequality", and that the links should be drawn more explicitly than in Glasgow between general issues (both "scientific" and "political") of inequality and the question of the health of the unemployed. One speaker, Chris Pond (director of the Low Pay Unit) remarked:

... there is a close link between unemployment and low pay. I am critical of some of the work on unemployment and health as it sees unemployment as something discrete. ... Really there is a spectrum from the casual worker to the well-paid, permanently employed person with a whole range in between. So the [Low Pay] Unit can't afford to ignore unemployment.

This theme was also reflected in other contributions to the Consultation.

One speaker, Prof Bernard Crick, had previously written "Certainly, if there was no difference in the death rate between social classes, we would know that we no longer had social classes." (In Defence of Politics, 2nd edition, 1982, p. 225). He told the Stirling audience:

The Black Report has established that ill-health is bad enough for the lowest social classes compared to the higher classes even when in jobs, but unemployment is associated with poverty not merely absolutely but also relatively. There may be no direct evidence that unemployment creates ill health, but the evidence is overwhelming that poverty creates ill-health. (Contact no 76 1983:3 p 12)

In David Player's contribution, he concluded "...those of us in the NHS should try to do something about social class inequalities in health along the lines recommended by the Black Report ... If social class inequalities in health could be tackled many unemployed people would benefit." (Contact no 76, 1983:3 p 8).

This might seem to portray the beginnings of a promising alliance between political scientists, "neo-Keynesian" economists (as Pond described himself) and a segment of the medical profession represented by Player. That Pond had hoped (and feared) for this is reflected in his reactions to some backstage activity at the conference.

I was expecting a hostile reaction [from the audience] because they were medics, and because I had had a minor row with Crick the evening before. He pooh-poohed the suggestion that one should look at low pay and unemployment on a continuum. It rather pulled the rug out from under me.

However, what the extracts from conference papers ("Contact" is the journal of the Scottish Pastoral Association and Clinical Theology Association) reflect is the outcome of a *conflict* within the steering group over the orientation of the conference.

In an informal discussion on 28 September of 1981, during the planning process for the Consultation, Adrian Sinfield had expressed concern at the lack of connection between those studying health and those studying the effects of recession on living standards and the labour market. Minutes of this meeting note that he had



instanced the separation of studies on unemployment, redundancy, and chronic poverty, also the split in academic and government departments between the various interests of health, economics, sociology and social administration.

These concerns come near to echoing (almost certainly unintentionally) Brenner's theme that unemployment strikes hardest at those already most vulnerable, and exerts its effects on health by increasing inequalities in both living standards and mortality. Although this theme was prominent in many of the papers, both those reported in "Contact" and those which were not, it was not reflected in the (rather scant) public reporting of the consultation. Nor was it remembered by many of the participants that I spoke to. The question of social inequality did not re-appear with any prominence in the unemployment and health debate until some three years later.

Some on the organising committee interpreted the references to the importance of poverty and inequality as an attempt by the "poverty lobby" to over-influence the agenda of the consultation. This, it was felt, might militate against unemployment and health being seen as a vehicle for enrolling "responsible medical" opinion. To accomplish this aim was important in two ways to academic social scientists involved with health issues:

- in order to consolidate their OWN claims to respectability and expertise
- for some, there was the additional wish to mobilise doctors in a campaign of criticism of government economic policies.

The second objective was shared with the "poverty lobby". What seems to have made the difference was that the first was not. As one participant saw it:

The social consequences of unemployment are hard to measure, but medical data is different, lay people trust it more, it has more legitimacy ... I thought it was very important to use doctors and epidemiological information -- it would be more effective if we had doctors ... than the same old, tired battery of activists ... I wanted a new medical dimension -- people who were uncontaminated by identification with a particular political line [such as] the left wing crowd from the Study Commission on the Family and the Low Pay Unit.

Only much later was it to be suggested that consideration of wider questions concerning social inequality might be a way out of the academic impasse into which the debate now descended (see section 8.2, and for an interpretation of the research using this approach see Bartley 1988). For the

time being, any association between those promoting the involvement of the medical profession in the social problem of the health of the unemployed and those who had for long been analysing and commenting upon inequalities in, for example, income, working conditions and security of work was seen as undesirable by the former group.

By seeking to make “political” considerations an explicit topic for the Consultation, some members of the planning group were violating one of the most powerful reasons why science can be used as a form of “politics by other means, that is, precisely by constructing value-claims as knowledge-claims, in this case as “medical data” which “has more legitimacy”. As Latour has observed:

Whilst in politics both the *official* (legislative, executive, judiciary) and the *officious* (compromises, cocktail parties, “arrangements”) processes of decision-making are recognised, in science it is still believed that no decisions are taken at all. Rather, science is held to proceed according to superior knowledge, a sort of divine right, which in essence escapes from the sordid negotiations of politics.

In addition, the introduction of questions about inequality and the operation of labour markets into the discussion would have produced a more complex and less topical and media-friendly approach (given the failure of the Black Report to generate very much public debate or policy activity). It was the simple link between unemployment and mortality which had both “inter-ested” the medical profession and gained media coverage. By diverging from this theme the organisers of the Stirling conference, despite the generally acknowledged quality of the speakers and their contributions, took the risk of reducing any possible impact it might have on the public debate, and thereby on policy-makers’ views of “what research was needed” at that stage.

This chapter has given an account of the “political” phase of the debate, in which the social-problem claims were raised onto the political agenda by way of publicising the knowledge-claims formulated by Brenner. It has shown the treacherous nature of this kind of exercise, which tends to attract opponents as well as allies, and the failure of Brenner’s claims to survive the first trial of strength unscathed. It has also shown the failure of a potential enrolment between groups such as the Low Pay and Unemployment Units (whose staff included “neo-Keynesian” economists) and reforming physicians which might,

even using the same material as Brenner, have produced a different type of account of the problem. However, "the health of the unemployed" had now become a topic, and even the activity undertaken to prepare an "official response" had the effect of interesting a new set of researchers who were seeking "policy-relevant" topics to work on. The next chapter will show how this happened.

## CHAPTER 5

### THE SECOND STAGE: MORAL AND TECHNICAL FRAGMENTATION

Chapter Four has reported the beginnings of the process which created "the health of the unemployed" as a significant problem of "interest" to both the media and researchers. It has shown the ways in which members of professional and disciplinary subgroups became involved in pressure-group activity and were "enrolled" in alliances with political groups seeking to promote the cause of regional autonomy in Britain.

This chapter shows the unfolding of Spector and Kitsuse's second stage in the history of a social problem, that is,

1. the development of the "official response" to a claim which has successfully gained the attention of the media and of political and pressure groups,
2. the beginnings of an involvement by researchers active in other fields, some of which would not immediately seem directly related to "unemployment"

These two processes ran in parallel: an explicitly "contracted" piece of work which threw serious doubt on the methodological adequacy of Brenner's paper; and a series of reports from ongoing studies which had not been set up to investigate the health of the unemployed. The combined effect of these was to produce what Manning would call a "loop" (see chapter 1.2), a circular argument into which the issue as source of public concern and debate tended to disappear. In order to understand how the latter process happened, it will be necessary to see how the studies involved (the non-"contracted" ones) THEMSELVES emerged from previous social-problem processes, and the way in which this influenced the sort of "answers" they were able to supply. This will be discussed in section 5.2. In this way, it will be possible to trace both the tendencies towards the "moral" and "technical fragmentation" of the issue<sup>24</sup>, and the acts of resistance to these tendencies.

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<sup>24</sup> Taken here, as explained in section 1.2, to mean the "individualisation" of the "health problems of the unemployed" and the construction of the question as something so "complicated" that only experts could understand it.

## 5.1 Opening the black box: the “official” response to Brenner

In November of 1980, following a tour which included testimony to the House of Lords Select Committee on Unemployment, Guardian economic correspondent Frances Cairncross interviewed Brenner. The resulting article was written in a sceptical vein. Cairncross was obviously well aware of the work in progress at Queen Mary College, although this was not to be published for almost another year. She also quotes research from the Policy Studies Institute showing that 90% of all people who became unemployed in the year 1979 had left the register within 6 months.

Those who remained out of work for long periods could almost be predicted in advance: they tended to be older workers, unskilled, with poor mental or physical health. They are precisely the people who, for a whole host of other reasons, would tend to have exceptionally high sickness and mortality rates.

This concentration upon the question of “who, in terms of health, ARE the unemployed?”, referred to by one DHSS advisor as “the *ceteris paribus* argument”, was to figure prominently in the future progress of the debate. Yet set in temporal context it can be seen that Brenner’s work in itself did not justify such an emphasis. In the autumn of 1980, NO-ONE had produced an academic paper claiming to demonstrate that individual unemployed persons were adversely affected in health. As an economist herself, it may be that Cairncross had access to information on how certain economists intended to steer the debate, and her relatively small article, placed in an inconspicuous position in the newspaper, marks an important turning point.

USHP economist Howard Cox remembers that:

When [Brenner] made that return visit, his work was already coming under fire. The main criticism seemed to be that he looked at an atypical time period. I told him I didn’t think it would wear.... I was conscious of a critical attitude the first time I mentioned Brenner’s name in a group of economists.

Whereas 1981 may perhaps be regarded as the high point of the public debate and of Brenner’s influence upon media comment, political controversy and government response it was also the period in which credibility in Brenner’s work was steadily undermined. It may have been knowledge of the forthcoming Cardiff conference (see section 4.2.4.) which precipitated a

statement by undersecretary of state for health George Young to the Conference of Northern Region Community Health Councils on April 3. Young's speech was the first example of a careful information subsidy produced by the Department of Health under a Conservative administration: press-released and full of promises of "research results". It was headed:

**"Possible Link Between Unemployment and  
Ill-Health Deeply Concerns Ministers"**

and went on:

Sir George said that two relevant research projects were commissioned by the Department some time ago and a third had been commissioned more recently. "I understand they [the results] are likely to suggest that there is little or no causal relationship between unemployment and health . where the duration of the unemployment is less than one year ... The median period of unemployment for the individual who goes on to the register is still comparatively short, about 3 months. So far as possible, we should be aiming our help at the minority who reach ... the third stage, when depression sets in: these are often the people who are at a disadvantage already, perhaps for health reasons, or perhaps because they are old, or unskilled.

By 7 July 1981 (provoked by a somewhat heated exchange in the House of Commons over the Fagin Report, as described in section 4.2.3. above), the backstage progress of Brenner's collaboration with his new found British enthusiasts was appearing in the public forum once again. Nigel Duncan wrote in PULSE:

For a long time the field has been left largely to an American research scientist, Prof Harvey Brenner of Johns Hopkins University ... But there have been a number of criticisms of his work ... The DHSS is now sponsoring its own research at Queen Mary College to see if it can repeat Brenner's findings ... But the findings will only show associations between unemployment and death rates. They cannot show causality, the effect of unemployment on health, and it is this which many people believe is of far greater importance.

In the "News" section of the same edition of the journal, Duncan wrote of suspicions that the DHSS was deliberately trying to muffle debate on the issue by limiting the availability of the Fagin Report. On the 23rd July, The Guardian and New Society referred to the unemployment and health issue. New Society commented:

There hasn't been a lot of critical evaluation on [Brenner's] findings. Some people have been concerned ... that, should Brenner's work not be substantiated, politicians who wished to play down the effects of unemployment would have a field day.

On 1 August 1981, the Lancet published a letter by Brenner which included figures for England, Wales and Scotland: Referring to the major critique which the group of economists were developing, to the effect that the equations used to describe the 1936-76 relationship failed to predict when applied outside of this specific time period. Brenner promises work on "trends in mortality for several major causes in England and Wales and Scotland during the period 1950 to 1978" which "confirm the main conclusions of the earlier report on England and Wales ... for the post-war period in England, Wales and Scotland, unemployment rates, specified for age, sex and duration of unemployment are strongly associated with increased death rates" (for the eventual results of these studies see Brenner and Mooney 1982, 1983, Brenner 1983). Howard Cox wrote to Brenner that this analysis would "make Gravelle et. al.'s critique of your earlier paper redundant."

The results of the Cohort Study health questionnaire were officially press-released on 24 Sept 1981 by the DHSS. It reads:

This is the first British study to examine this possible link using data from a national survey ... and accepts that an association [between unemployment and health] is not disputed. What is disputed is whether unemployment itself causes ill-health. Unhealthy people may well be more likely to lose their jobs and have difficulty finding a new one: the worse the sickness record, the less the chance of re-employment.

The paper advertised in this press release (Ramsden and Smee 1981) illustrated these claims with figures on the health of those members of the DHSS Cohort Study who had remained unemployed during the whole period of the study. This group's self-reported health did not appear to have been any more likely to deteriorate than the health of those re-employed.

On 26 September, the Lancet published "Mortality, and Unemployment: A critique of Brenner's time-series analysis" by H.S.E. Gravelle, G. Hutchinson and J. Stern. Brenner's work, they argue,

is important because it suggests that the social costs of unemployment may be higher than has previously been thought. Further [they claim, somewhat surprisingly] time series analysis

CAN provide a fairly precise estimate of how much mortality rates would rise following a given increase in unemployment.

The first section of the paper concentrates on (a) the 1930s studies and (b) what Brenner had claimed in Cardiff on April 10.

Stern disputes the point that "high unemployment is associated with declines in real income for the employed as well as the unemployed." This scholarly if brief review then tackles the problem that CROSS SECTIONAL STUDIES tend to SUPPORT the notion of a correlation between measures of unemployment and mortality in different geographical areas. However, Stern reminds the reader:

these studies typically find that a number of other variables such as income, occupational structure, educational levels, consumption patterns and housing are also associated with mortality and that these variables are strongly correlated with unemployment rates. If these other variables are included in the analysis, the reliability (in terms of standard errors) of the estimates of the effect of unemployment will be reduced... On the other hand, if these other variables are omitted, the estimates will be biased, in that some of the effects of omitted variables on mortality will be wrongly attributed to unemployment.

The tone is set here for all the subsequent attacks on Brenner's work. Although the rest of the paper is highly technical (as was Brenner's original paper), no detailed account is necessary in order to understand the role played by the economists' arguments. The task they performed, despite the complexity of their papers throughout the debate, was not one of "technical" but rather of "moral" fragmentation. In essence, they argued that people who were found to be unemployed were always "different" in other ways to those employed, and that this "difference" resided in essentially "moral" characteristics such as health-related behavior, intelligence, and other aspects of "human capital".

The argument contained another strand as well. This was the importance of the "once for all" change in both mortality rates and unemployment rates which took place at the end of the Second World War. In statistical terms, these two sharp changes were so large as to determine the value of the equation linking mortality and unemployment, so that if one attempts to apply the same model to time periods which exclude the postwar period, or which split the period 1936-1976 in two, it fails. In political terms, this argument marks out the professional territory of the economists. They were arguing, in effect, that the welfare state had brought about a decisive improvement in



health, such that any analysis of the much smaller changes in the periods before and after 1945 was of little relevance. Gravelle, Hutchinson and Stern conclude:

his [Brenner's] evidence does not support the hypothesis that aggregate unemployment rates have a serious adverse effect on population mortality rates.

Though cautioning:

Our results ... do NOT mean that unemployment has no adverse health effects. Indeed it is plausible that such effects do exist – but there is as yet no evidence which can be used to estimate their magnitude, timing and form.

The effect of these two papers on the public debate conducted by pressure groups was to damp it down very considerably. For example, by 30 Sept 1981 the Unemployment and Health Study Group had concluded that their previous (May 1981) enthusiasm for the idea of "mobilising the medical profession behind unemployment as a depoliticised health issue (as with nuclear weapons)" had been misplaced and that a "Medical Campaign Against Unemployment" was a "non-starter because there was no broad core of medical commitment as with nuclear weapons."

On October 3 1981, the ever-alert Pulse journalist Nigel Duncan reported:

A major new row has broken out over unemployment and ill-health and the government's role in the affair ... Both reports [Ramsden and Smee and GH&S] will be welcomed by ministers and will harden their controversial refusal to accept any link between ill-health and unemployment ... critics of the government [were] describing them as deliberate attempts to discredit the growing campaign which sees unemployment as a major threat to public health. [This] comes just 5 weeks before Brenner is due to produce a major new analysis on unemployment and health in Scotland ... DHSS officials hope that critical scrutiny of Brenner's work will inject an element of caution into the debate.

On 20 October a series of Parliamentary Questions were asked about Len Fagin and Martin Little's report (see section 4.2.3. above) and other research into unemployment and health which the government was suspected of manipulating. Gerard Vaughan told Dale Campbell-Savours that the work of the research team at Queen Mary College

calls into question the validity of the statistical model used by Professor Brenner since it fails to take account of the relevant

factors such as improvement in diet and medical care which occurred during the period. (Hansard 20 Oct 1981 WA col (10)97)

Gravelle et. al. had called Brenner's bluff in using his own complicated methods against him, they had, to some extent, broken open the "black box" (the time-series analysis). Up to this point, interviews with social scientists working in medical settings showed, the fact that few "really understood" Brenner's methods had not dissuaded them from using what seemed to be a persuasive example of the power and value of a social science perspective when applied to pertinent questions about health. In Latour's sense, the methods comprised a "black box", passed on co-operatively from person to person, because it could be used to further their several and various "interests". For example, it made impressive teaching material to present to sceptical medical students. It was this disciplinary entrepreneurialism which provided the "respectable" academic backing to the "Celtic fringe" conferences discussed in section 4.3. However, such success can also have its costs, in that it attracts powerful ENEMIES as well as allies.

Gravelle et.al.'s response to Brenner and the "health" paper from the DHSS Cohort Study (Ramdsen and Smee 1981) therefore comprised the "official response" to the use made by "Manifesto" community medicine and pressure groups of Brenner's work. The failure of the DHSS to produce a more formal version of the Fagin report (and the fact that there only 200 were made available), and Ministerial denial in the House of Commons that the report demonstrated any "causal" effect were taken, by some participants, to be another indication of official opposition. Also, media interest was, at this time, beginning to shift from the effect of unemployment on health *itself* to the (sometimes thrillingly unseemly) conflicts between the medical profession and ministers, and the associated accusations of "cover-up".

## **5.2 Stage two (B) – Setting up the loop**

This section will be concerned with the ways in which an "epidemiological" response to Brenner's work and the concern it aroused was constructed. It is at this point that some concept of a "loop" (see Ch. 1) is needed. The policy debate now appears to "dwell", awaiting the outcome of "research", and this

hiatus lasted a considerable time. The nature of the early academic response was shaped by the fact that there was felt to be no time to await the outcome of new studies, so that “customers” were asking researchers for relevant work based on existing programmes. This section describes the three research programmes which produced information that was fed into the unemployment and health debate, and attempts to understand their contribution in terms of these programmes’ relationship to *previous* social-problem processes. They will be discussed in the order in which their first published work on unemployment and health appeared.

### **5.2.1 The British Regional Heart Study**

The initial impetus for the setting-up of this major prospective study of coronary heart disease might appear to have been derived from a research programme far removed from the field of health inequalities or unemployment. The rationale for the initial MRC grant in 1975, was the investigation of the relationship between heart disease and hardness of water supplies. And yet, the history of the MRC Social Medicine Unit, from where the research proposal originated, tells a different story.

In 1948, one author of the pioneer epidemiological paper on health and recession (Morris and Titmuss 1944), J N Morris, was appointed Director of a new MRC Social Medicine Research Unit at the Central Middlesex Hospital. Early projects included the investigation of biological and social factors in infant mortality, of social and psychological factors in reproductive efficiency, and the occupational incidence of heart disease in middle life (Committee of the Privy Council for Medical Research, 1949, p 180). An early member of the staff was Richard Titmuss, co-author of the 1944 paper. On Titmuss’ death in 1973, the annual report of the council went so far as to state, in his obituary, that working as Deputy Director of the Social Medicine Unit “provided the stepping stone to his academic career” (MRC 1975, p. 153).

In the academic year 1955–6, the units staff was joined by Dr M D Crawford. In 1961, she published, in association with Morris and J A Heady, who in that year became Assistant Director of the unit, a paper on “Hardness of water supplies and mortality from cardiovascular disease in the county boroughs of England and Wales” (Crawford, Morris and Heady 1961). In 1962–3

Peter Draper joined the Social Medicine Unit, and stayed until November 1964. During this period the basic research programme changed little. It was not confined to cardiovascular disease, but included work on juvenile delinquency, mental illness and pollution of air and water, but heart disease seems to become a more dominant concern. On 30 September 1967 Morris was appointed to the Chair of Public Health at the London School of Hygiene and Tropical Medicine, and the Social Medicine Unit moved to Keppel Street. It seems that Crawford's work on what came to be known as "the water story" was the most successful aspect of the unit's work at this time. In the School's annual report for 1968-9 (London School of Hygiene and Tropical Medicine, 1969) some prominence is given to it, and to an account of further work planned on mortality in 12 soft water and 12 hard-water towns. This classical public-health initiative was to be undertaken in collaboration with the local medical officers of health.

In January 1970, Dr A G Shaper, who had previously been Research Professor in Cardiovascular Diseases at Makerere University College, joined the staff of the unit. The MRC's annual reports from 1971-2 onwards betray new concerns following on the implementation of the Rothschild reforms, which have been discussed in chapter 3. It does appear that concern with "arterial disease" and the causes of the kind of change and damage in blood vessels which is thought to eventuate in heart attacks was seen as a topic which would appeal both to scientists and to "customers" in government.

By 1970, also, the Hunter working party on the reform (some would say the destruction) of Public Health (see Lewis 1986b) and the development of the new "community medicine" had been set up. Morris, still Director of the Social Medicine Unit, was heavily involved in this, as were many other members of School of Hygiene staff. In this ferment of change in both health policy and science policy, the "water story" may have seemed an attractive investment from several different points of view.

In the words of member of the British Regional Heart Study staff, it seemed that this might pose a "classic public health problem" -- that is, water quality might turn out to be the sort of aetiological factor which could only be affected by "mass intervention" of a type which would have to be sanctioned by government (the most "successful example of this was perhaps the Clean Air Acts; another example of such an issue would be water fluoridation to

prevent tooth decay.) It also involved combining epidemiological skills with the more traditionally highly valued clinical skills which were needed to frame hypotheses about the possible pathways by which water constituents might affect the heart, and to integrate the results of case histories, blood chemistry, electrocardiography, spirometry, etc.

In 1975, the Social Medicine Unit was wound up, on the retirement of Morris. As funds had been obtained from the MRC for the Regional Heart Study in the shape of a five year "Programme Grant", Shaper, who became study director, moved, project and all, to the medical school at the Royal Free Hospital, where he took up the chair of clinical epidemiology. The design of the Regional Heart Study reflected aspects of the method adopted by Morris and Titmuss in the 1930s (Morris and Titmuss 1944, see also discussion by Bartley 1987a), and continued by the work of Morris, Crawford and Gardner at the Social Medicine Unit. Twenty-four towns were chosen to represent all combinations of water quality and heart disease mortality -- so that investigation could be made into the "exceptions": those towns with high heart disease and hard water and vice versa. Approximately 350 men between the ages of 40 and 59 were interviewed and physically examined in each town between mid-1978 and mid-1980, and the quality of drinking water was tested from the homes of a 10% subsample of these. As well as lung function and blood lipid analysis a questionnaire was administered to all men. The questionnaire collected details of present state of health (so that the researchers knew not only what diseases, if any, each man was suffering from, but also where he and his doctor KNEW about them), diet, exercise, smoking, drinking and occupation. Occupation was not coded according to present job, but according to longest held occupation. As a more or less accidental product of this exercise, 408 men were found to be unemployed, most of them (according to their own accounts) "because of ill-health".

At the beginning of the academic year 1980-81, the team was joined by an American Millbank scholar, doing the practical work for an M. Sc. in Epidemiology at the London School of Hygiene, Richard Cummins. Cummins was from Arkansas, had graduated from the liberally-inclined Case Western Reserve medical school, and worked in programmes designed to bring medical care to underserved poverty areas. He did some work on smoking and drinking behaviour, and in the process teamed up with a GP who was doing some "sessional" teaching in the Department, Ray Hume. Hume was particularly

interested in tranquilliser use, in particular the idea that if patients were withdrawn too quickly from tranquillisers, they might turn to alcohol. Running through the data, they discovered that an unexpectedly large number (in view of their age and gender) of the study's subjects were taking tranquillisers. It also appeared that one factor significantly associated with tranquilliser use was unemployment. At this stage, Hume and Cummins enlisted the help of one of the statisticians working on the study, Derek Cook.

The results of running employment status against health variables, at first sight, were quite striking. Thirty-one percent of the unemployed, as compared to 15% of the employed men had bronchitis, 28% of the unemployed and 15% of the employed had obstructive lung disease, and 26% of the unemployed, in contrast to 9% of the employed had chest pain indicative of ischaemic heart disease. It was then necessary to allow for the fact that so many of the BRHS men without jobs were unemployed BECAUSE of their ill-health. Doing this changed the picture considerably. The "not ill unemployed" were far less likely than the "ill unemployed" to be suffering from symptoms suggestive of heart and lung disease. There was still some difference between the employed and the unemployed, though this was further decreased when statistical adjustment was made for social class, town of residence and smoking. However, even then, men unemployed not because of ill-health were still significantly more likely than the employed to have chest pain indicative of heart disease.

A version of the paper was presented at the BSA Medical Sociology Conference in September. It caused little comment. Cook then continued to work on the draft with the study director until they were satisfied, in late March 1982, and submitted it to the Lancet. It was accepted without major alteration, and published on 6 June. Somewhat to Cook's surprise (and disappointment), the appearance of the paper caused very little media comment, a few column inches on an inside page of The Guardian.

### **5.2.2 The OPCS Longitudinal Study (the "LS")**

On 9 June 1982, the monograph "Socio-Demographic Differentials in Mortality" appeared, published by Her Majesty's Stationary Office. It did not provoke any great media response, although there was a certain amount, for example a Times piece on page 2 headed "Unemployed Have Higher Death Rate

-- Study Shows" by Nicholas Timmins. It reported that the results showed a death rate from accidents and violent causes, including suicide, amongst the unemployed well over twice as high as that for men in work, and a death rate from cancer half again as high as that for the employed. It should be noted here that the article in the Times more or less allows readers to define for themselves what is meant by "the unemployed". In fact, "unemployment" in this study is operationalised as "seeking work' in the week before Census night 1971".

It is worthwhile giving some fairly extensive background information on the LS, in view of its major influence in the debate on unemployment and physical health in England and Wales. Traditionally, measures of mortality differences between occupations and social classes are given by The Registrar-General's Decennial Supplements (DS) which accompany (usually at considerable delay) each Census. By 1971, the Office of the Registrar General had been merged with the Government Social Survey to form a new Office of Population Censuses and Surveys.

For a considerable time, misgivings had been expressed as to the adequacy of existing methods for the measurement of social class differences in mortality. The technical problem arises from the use of different sources to establish the numerator and the denominator when calculating mortality rates. The denominator (i.e. the numbers in each social class) is derived from the census count. The numerator (the number of deaths occurring to members of each social class, usually in a three-year period around the Census date) is taken from the occupations stated on death certificates. As discussed by various commentators (Leete and Fox 1977; OPCS 1986 p. 12-17) this gives rise to the risk of "ecological fallacy" and "numerator-denominator bias". Leete and Fox, for example, found that of the 89 male members of social class V who registered a birth in 1971 (a Census year) after enumeration day, over 49% were allocated to a higher social class on the basis of the occupation that they stated at the registration of their child than they had been at Census.

In 1973, OPCS Report on Medical and Population Subjects No. 25 (OPCS 1973) "Cohort Studies -- New Developments" had suggested a way round this problem. It stated that:

chronic diseases, which occupy a large and increasing proportion of the facilities and cost of the health service, develop over many

years ... with present-day job mobility many persons exposed to an occupational hazard adopt other occupations before their disabilities become apparent (p. 5)

The new kind of cohort study that it was proposing, the Report pointed out, would "take statements of occupation as given at Census both for the numerator (those who die) as well as for the denominator (the population at risk). This study will also allow a comparison to be made of the statements of occupation given at Census with those given at death registration, and so will provide a measure of the errors introduced in consequence into the traditional methods of measuring occupational mortality."

According to a senior government research manager, the first initiative which led to the setting up of the LS "came from the General Register Office" because of concern about the comparability of information about social class obtained at death registration and at Census.

It is the business of this office to produce classifications which are meaningful and useful, and concepts of what is meaningful and useful change ... The specific debate in 1973 was the validity of the [social-class-specific] mortality rates. ... It could also be seen as efficient [to set up an LS], because it made use of existing data in new ways. [Nevertheless] there were formidable problems of organisation. It is not a cheap thing to set up a file on half a million members of the population and add to it year by year. ... You do have to have a very good story to justify these longitudinal studies being set up. Our story was ... traditionally we have produced social class differentials [in mortality and fertility rates] so we could say that we could do our job better with a longitudinal study.

The idea of a longitudinal study of one percent of the population had also been linked to moves towards quinquennial Censuses, and a growing interest in "record linkage" as in aid to population surveillance and planning<sup>25</sup>. In 1966 a 10% Census was carried out, and plans for a 1976 Census were only cancelled at the last moment (Whitehead 1984). So, in the early 1970s, it had been thought that there would be a FIVE-yearly input into the LS, which would have allowed access to much greater detail on the succession of events occurring to

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<sup>25</sup> see Acheson 1967, 1968; Dr. Donald Acheson later became Chief Medical Officer at the DHSS and chaired the committee on the future of community medicine and the public health function set up in 1986.



sample members. As one administrator put it:

In the early 1970s, you see, planning was all the rage, and we felt that planning purposes required more frequent Censuses.

The task of following one percent of the 1971 population, linking Census records to records of births, deaths and cancer registration, and then on to 1981 data, did prove formidable. It was not until 1975 that the 1 percent sample of the 1971 Census had even been assembled, and no reports appeared until 1979. The sample was drawn by choosing 4 days in the year and including anyone born on that day (because  $4/364 =$  approximately. 1%). In all subsequent years, new births or immigrations taking place on any of those four dates were added to the sample. In principle, data on each individual from each Census would be added. This meant that occupation and economic position would only be available every ten years. The study could really only claim, therefore, to measure long-term changes in occupational status or employment status, and was unsuitable as an indicator of short-term mobility within, and in and out of, the labour market.

In 1975 a young statistician, John Fox, was recruited from the Health and Safety Executive to write the Decennial Supplement on Occupational Mortality to the 1971 Census. While still working on the DS (OPCS 1978), Fox became interested in the idea of the LS and began to assemble some resources to push the analysis forward. In 1979, he argued that a team of full-time personnel would be needed. Many at OPCS agreed with this, but at the time of restraint on civil-service recruitment, there would have been little chance of staff being allocated to the study<sup>26</sup>. The method by which staff are recruited into the OPCS's Medical Statistics Division (of which Dr A M Adelstein was Chief Statistician, and in which Fox and the co-author of "Socio-Demographic Differentials in Mortality" Peter Goldblatt worked) would also not, perhaps, have been appropriate to attract the sort of colleagues now needed. Recruits into OPCS (in contrast to the Government Economic Service) can only be taken from the Government Statistical Service by internal selection. "The best we could have got that way" remarked one LS worker "was a numerate

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<sup>26</sup> The "Rayner Review" of staffing in the Government Statistical Service was being carried out during this period, reported in 1980 (Rayner 1980) and resulted in a White Paper in 1981 (HMSO 1981). It recommended that the GSS be cut by 25 % in real terms between May 1979 and April 1984, see Hoinville and Smith 1982 for an account of the Report, and of statisticians' reactions to it.

administrator." So Fox decided to apply for a two-year Professorial Research Fellowship in the Department of Mathematics at City University, and asked OPCS if he could try and obtain outside funding to build up a research team. He obtained funds, eventually, from the Cancer Research Campaign (for a post which was taken up in 1980) and the SSRC (1981). The OPCS continued to cover the costs of half Fox's salary and the heavy costs of data processing. These were the resources which lay behind the production of the first monograph "Socio-demographic Differentials in Mortality" which appeared in 1982.

As early as 1979, Fox had published a paper in the *Annals of Occupational Hygiene* which included a table showing raised mortality amongst those men classified as "seeking work" in the week before the 1971 Census (Fox 1979). The paper went totally unremarked except by workers on the DHSS Cohort Study who were preparing the third-interview phase of their survey, and had decided to include a question on health, in order to make the Cohort Study's results relevant to policy-makers disturbed by *The Reckoning*. Prior to this time, contact between the two studies' personnel had, perhaps surprisingly, been low -- though one government statistician remarked that back in 1978 a DHSS scientific adviser had "offered the Cohort Study to OPCS and some idiot turned it down".

However, political events had changed attitudes temporarily (contact between the two studies did not last very long). There are two accounts of what happened next in my field notes: one given on a formal occasion (a presentation of results at an ESRC funded seminar on Employment and Unemployment) and the other informally. According to the formal account, "The Department of Employment and the DHSS approached us to see if we were interested in doing research on unemployment and health. However, in the end we went to the MRC, and other bodies, for our funding." According to the "informal" account, a professional adviser at the Department of Employment had suggested "over lunch" that they take another look at the data first presented in the *Annals of Occupational Hygiene* paper. Although this account displays the importance of informal networks, in this case the LS researchers decided not to proceed on the basis of mere social contact. They asked that the Department of Employment officially approach the Registrar General and make the request in writing. The Department of Employment never funded any work on the LS, and the DHSS did not do so until late 1985.

Those involved in the LS (in contrast to those involved in the BRHS) were well aware of the policy-making process in which public-health research is located:

Take the Decennial Supplement [one remarked]. It's done every 10 years. Why? Because it reminds people of the old things we already know about, but that would be forgotten about otherwise. Otherwise it might be assumed that all the inequalities had gone away.

The idealistic and ameliorative spirit of many involved in the collection and analysis of official health statistics is combined with a sense of the tradition in which their work is located (one factor providing links with the concerns of Manifesto community medicine). Another government scientist reminded me that:

We should continue to look at structural variables, and to make what assessment we can of what you can infer from the time-series. That is our traditional role ... Our role is to ensure that the data is of the best possible quality ...

However, and perhaps somehow rather contradictorily in view of the comments just cited, this civil servant insisted that:

We only provide background information -- fundamental information as it exists, and also what assessment we might make of that situation ...

And this line, between "information" and "assessment", was maintained by the "careful forms of words" discussed in Section 4.2.3.

It was in this spirit -- of a sense of tradition, concern for accuracy, and cautious awareness of the political role of the oldest "government research unit" -- the General Register Office -- that the OPCS LS reported on the health of the unemployed. The SMR for men of working age who reported themselves to be "seeking work" on Census night 1971 was 130, that is, their age-standardised death rate was 30% above that for all men. Fox and Goldblatt noted that "there are no clear health grounds for expecting this category to record high mortality" (Fox and Goldblatt 1982, p.26) and suggest that it may be an effect of the social class distribution of the unemployed, i.e. the tendency for unemployment to be concentrated in the lower and less healthy classes, or by the higher risk of losing a job experienced by men who are already ill. At this stage, answers to these two questions were sought by looking at the CAUSES of the excess deaths (which turned out to be accidents and violence

and cancer) with the results quoted in the Times. The high mortality from accidents and suicide was seen as evidence for a causal effect, and that from cancer as evidence for a "selection" effect (men already ill losing their jobs).

What the researchers did next offers evidence on the ways in which scientists operate as "analogical reasoners" (Knorr-Cetina 1981). A certain set of ideas about how the effect of work hazards on health should be studied were carried over from the field of occupational medicine into the Longitudinal Study. In studies of specific potential hazards, account has to be taken of what is known as the "healthy worker effect". By this is meant the consistent finding that mortality rates for ANY kind of worker are lower than general population age-specific rates, which include those not in the workforce at all. If followed over time, however, it can be seen that these low mortality rates return back to the level for the population as a whole, that is, in the jargon, the healthy worker effect "wears off"<sup>27</sup>. It is therefore considered necessary to choose, as a control group in studies of possible industrial hazards, not a sample of the general population, but a sample from another industry which may be regarded as similarly selected for GOOD health. Otherwise, the healthy worker effect may conceal the effect of the hazard. One promise of the LS was that it would allow the healthy worker effect to be compared across many different occupations. However, by 1981, only deaths up to 1975 were available for analysis, and not enough deaths had taken place to permit statistically meaningful results to be obtained on individual occupations. Instead, its effect on the differences in mortality between men in different economic POSITIONS was examined.

The effect is duly observed in the LS sample, with, between 1971 and 1975, the 3021 deaths of men employed on Census night 1971 giving an SMR (ratio of observed deaths to those expected if the death rate of the employed had been the same as that for "all men" -- i.e. employed, unemployed, retired, permanently and temporarily sick, and students) of only 86. The healthy worker effect was so strong that in the first 5 years following the 1971 Census, all members of the LS sample allocated to ANY occupation experienced low mortality rates in comparison to those found by the ecological method used in the Decennial Supplement. This is because at death registration, very few people are left in the residual category "unoccupied". Unemployed and

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<sup>27</sup> For a clear explanation of this idea, see Carpenter 1987.

permanently sick men are usually given an occupation on their death certificate, generally the last one they held, or which the informant at registration remembers. As a result, in the LS, where men were classified at death according to their occupation and economic position AT CENSUS, there were MANY MORE deaths to "unoccupied" men. And the categories "unoccupied" and "inadequately described" were found to have very high mortality indeed. Because these men were not included in the LS analysis of mortality by occupation or social class, the gradients in mortality for the period 1971–1975 were far flatter than those found in the Decennial Supplements and used in the Black Report.

Now, this might at first have appeared as a blow to the Black Report's contention that inequalities in health were still a significant policy issue for the 1970s and 1980s. An account of how the results of the DS entered the wider debate on health inequalities is given elsewhere (Bartley 1987b). But what was important for the unemployment and health debate was to be the use made by Fox and his colleagues of the notion that, like the "healthy worker effect", the "selection effect" (of workers already in poor health out of the labour force) could be regarded as likely to "wear off". Their interpretation of what their data could show about the effect of unemployment on health hung crucially upon this analogical move between occupational and general-population epidemiology.

### **5.2.3 The work of the MRC Unit for Epidemiological Studies in Psychiatry**

The study whose results were reported last of all was probably the one which had the greatest media impact. Like the other two, it was funded by the "independent" MRC and carried out by full-time researchers (i.e., academics with minimal teaching commitment). The authors were two members of the MRC's Scientific Staff in a long-established research unit in Edinburgh. It will be worthwhile, in this case, also, to give an account of the development of the Unit's work, which provides an example of the way in which another scientific team become involved in "social problem claims making".

The Unit began to operate in April of 1960 in London, directed by a psychiatrist, Prof G M Carstairs. In January 1961, Carstairs was appointed to the Chair of Psychological Medicine at Edinburgh University and, as is the

custom with MRC Units, this one moved (in April 1961) with its Director. The interests of Carstairs and his Assistant Director (W I M Kessel) did not explicitly include unemployment as a specific topic. However, the unit's ethos partook of the rationalist and optimistic spirit of the time in regard to health planning:

Epidemiological studies have two principal purposes [said an early progress report]. The first is to explore aetiological factors in mental illness; the second is to estimate needs and evaluate treatment services ... the former will in the long run provide information enabling prevention and treatment to become rational instead of empirical (Unit Progress Report for 1960-64, 21 May 1964, p. 1)

From the start, the Unit formed a link with the Edinburgh Regional Poisoning Treatment Centre (RPTC), at this time a ward (Ward III) of the Royal Infirmary, which received over 90% of all cases of self-poisoning and self-injury in the city that required hospitalisation. The first study of the aetiology of what was later to become known as parasuicide (the phrase "attempted suicide" was still in use at this time) explored the clinical, social, demographic and ecological characteristics of cases -- this included social class but not employment status. There seems to have been a plan at this time to set up RPTCs in all major British cities.

Dr Norman Kreitman joined the Unit's scientific staff in January 1966, and became Director in April 1971. By this time, studies of suicide and "attempted suicide" took a prominent place in the publicly presented image of the Unit's work. Parasuicide was seen as being "on the increase", especially amongst young women, and was found to be correlated with overcrowding, juvenile delinquency, divorce, and residence in tenement dwelling, both at the ecological and at the individual levels. A member of the staff, J W McCulloch, wrote an M. Sc. thesis on "The social consequences of acts of deliberate self-poisoning or self-injury" (Edinburgh 1965) in which "particular attention was paid to the patient's status at the time of admission, with respect to age, civil state, marital adjustment, social class and work record, finance and housing.

By November of 1971, studies of completed suicide and parasuicide had become the Unit's major research activity. One hypothesis now being explored was that certain "problem" areas with high parasuicide rates were characterised by a "subculture of parasuicide" in which self-destructive behaviour was

regarded as normal or acceptable (Unit Progress Report November 1971, p. 3) The "social problem" of parasuicide continued to grow. Rates of parasuicide amongst young men had trebled between 1962 and 1967. By 1973, annual admissions to the Regional Poisoning Treatment Centre had risen to 1650, from 860 in 1967. Rates for men had risen from 179/100,000 to 296/100,000 and for women from 209 to 427 in the same period. In 1968 a Ph D student, Alex Robertson, had joined the Unit. His study looked particularly at young working class male parasuicides, and found that they differed from controls in "frequency of recent stress, hostility and depression scores, and sense of relative deprivation" (Unit Progress Report October 1974, p. 7). It seemed that these young men had been subjected to a particularly high frequency of life events.

Stephen Platt joined the Unit in June of 1977. The social problem status of parasuicide had been maintained up to this point ("Between 1968 and 1976 annual admissions have almost doubled" according to the Progress Report of July 1978) and the recording system at the RPTC had become even more sophisticated. Platt started work on the "subculture" theory for his Ph D. But by June 1981, the Unit was beginning to suffer from a contraction in research funding. Only one project on suicide remained. Staff vacancies had been frozen and staff members had been reduced by around 20% between mid-1980 and mid-1981. The priority in research was now alcohol and alcoholism. The progress report for 1978-1981 stresses that:

The RPTC ... probably represents the best facility in the world for the study of hospital-referred parasuicide, and it is regrettable that this resource cannot be more fully exploited. Parasuicide is still a scientifically fertile area and poses major issues for health service provision and clinical management

However, in the period 1977-1979 all three rates of parasuicide (person, event and first-ever) had been falling by 10% annually. This trend puzzled Unit staff, as "the changes [among males] were largely due to fewer short-term re-admissions and a decrease in 'acute situational reaction' presentations, despite increasing economic recession and unemployment in the community." They seem to have been unaware of a temporary fall in the rate of unemployment during 1977 to late 1979. The Unit's progress report for 1978-81 admits that if a choice were to be forced upon it, studies of suicidal behaviour would have to be placed fourth (and last) in its order of research

priorities.

Like Leonard Fagin, Steve Platt's interest in unemployment derived from a more general political orientation which had developed during experiences in South America. He had lived and worked in Allende's Chile for 18 months after completing a Master's degree in social policy at the LSE. His work with Chilean psychiatrists committed to alternative humanistic forms of treatment deeply influenced him. By mid-1981, his subculture study was beginning to show that in the high-rate areas of Edinburgh the local subculture seemed to express, if anything, a MORE punitive attitude towards self-destructive acts than that found in low-rate areas. During 1981, Norman Kreitman had come under some pressure from the MRC to develop work on unemployment in the Unit, but no definite plans were made, as few staff had any interest or expertise in the area. However, as part of his work on parasuicide, Platt had been using data from the RPTC. When discussions about unemployment resulted from the 1981 review of the Unit's finance by an MRC subcommittee, he remembered that data collected on each admission included employment status. At around this time, Unemployment and Health Study Group member Gill Westcott wrote to the Unit asking if anyone there would be willing to give a paper on unemployment and suicidal behavior at a WHO conference to be held in Leeds in December 1982. "That" Platt recalled "gave me a deadline". As he looked at the possibilities of the RPTC data he became increasingly excited by them. He thought that they could both throw new light on structural factors in parasuicide and, in more abstract methodological terms, provide an opportunity to carry out an epidemiological exercise combining individual and aggregate levels of analysis.

The transfer of ideas from his previous work to unemployment was different in Platt's case to that in Fox's. Whereas Fox carried over the model of the healthy worker effect by analogy, Platt was attracted to using unemployment as a variable in the study of parasuicide precisely because it was so DIFFERENT from the "cultural" factors involved in his PH D. (Platt 1984 p. 74-5). For some time he had been worried about the potential for misuse presented by any "subcultural" theory of social phenomena. Any theory on a subculture of parasuicide could, he felt, fall into this trap. The ideas of Henderson (in, for example, Henderson and Williams 1974) seemed to him to suggest potentially that parasuicide should be "re-criminalised" in order to combat it, and had gained, Platt felt, unjustifiably wide currency and credibility



both in academic and policy circles. He remembered the ways in which Oscar Lewis (who also wrote about Latin American society) had been “misused” in the “culture of poverty” debate in the United States to justify neglect of the problems of the urban poor, and to distract attention from “structural” causes of poverty. In preparation for work on unemployment (a “structural” factor) as it related to parasuicide, he began to write an extended literature review of the evidence relating unemployment to suicidal behavior. He presented a preliminary report of this work to the Scottish Medical Sociology Conference in the early spring of 1981. At the behest of a member of the editorial group, he then submitted a version of this paper to *Sociology of Health and Illness*, who turned it down, though it was eventually published in *Social Science and Medicine* (Platt 1984) and widely quoted. He then gave a small workshop at the Edinburgh Medical Group’s “Consultation” in Stirling (see section 4.3.3) in June of 1982. He felt that the conference had provided him with valuable contacts, but “none of this was ever followed up”.

In August of 1982, however, while Norman Kreitman was on holiday, BBC Reporting Scotland phoned for the Director’s comments on rising suicide and unemployment in Scotland, and his secretary, knowing the interests of other staff, passed the call through to Platt. His comments were repeated on August 12th in the *Glasgow Herald* (“Study Links Suicide with Unemployment”) and on 13 August in the *Scottish Daily Express* (“A Job to Survive”).

A short version of Steve Platt’s review of the evidence relating unemployment to “completed suicide” was published in the *Unemployment Unit Bulletin* in November 1982 (Platt 1982). The paper made no claims that unemployment necessarily exerted a “causal” effect on suicide. It was a cautious account of his research to date, very much in the model of MRC-funded work, despite the somewhat unorthodox nature of the journal. The Unemployment Unit, like the MRC (and notwithstanding its status as a “intermediate body” with the reputation of being “political”) placed high value on balance and impartiality, which it maintained despite its explicitly campaigning purpose. Platt concluded:

My own view is that the weight of evidence provides some ground for suggesting that the risk of both unemployment and suicide are elevated by the presence of psychiatric illness (especially depression), rather than that unemployment is an immediate cause of suicide. This is not to deny, however, that joblessness may indeed constitute an important intervening or

moderator variable affecting the strength of the association between psychiatric illness and suicide" (Platt 1982, p5)

This work on suicide laid the foundations for the analysis of the RPTC figures on parasuicide amongst the unemployed, which came to public attention during 1983 and will be more fully dealt with below. It claims no more than that unemployment is an "intervening" or "exacerbating" factor which comes between personal states and characteristics and suicidal behavior. As later described in the Unit's Report to the Medical Research Council for the quinquennium 1981-1986:

At an individual level the rates of parasuicide among the unemployed have declined, suggesting a progressive dilution of the pool of the unemployed with respect to personal pathologies. The relative risk for parasuicide of being unemployed varies according to how dominant unemployment is in various ... subgroups ... Psychologically, hopelessness rather than depression per se seems to be the important discriminator. This finding has led to a more general theory concerning the role of hopelessness and depression in parasuicide.

The last two sentences relate the work on unemployment back to the Unit's central concerns (which lie elsewhere)<sup>28</sup>. This synopsis also demonstrates the ways in which the research could be presented as "about" personal psychological tendencies in unemployed people, an interpretation which was later to prove the "undoing" of attempts to present the effect of unemployment on self-destructive behaviour as a "public health problem".

This section has described the origins and design of the three studies which provided the most important data used in the rest of the debate. It has been shown that adoption of "the health of the unemployed" as a social problem worthy of research was a move which emerged out of the research teams' previous participation in debates on quite different types of social/medical "problems". This move was encouraged by the senior researchers' perceptions of the need for the work of their "laboratories" to appear "policy-relevant" and "service oriented" in the light of the extension of the Rothschild principles into medical and social research, combined with

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<sup>28</sup> For recent examples see Ingham et. al. 1986, 1987.

restrictions on funding which made the “normal” types of scientific entrepreneurialism all the more important.

## CHAPTER 6

### STAGE THREE (A): EVIDENCE AND CIRCUMSTANCES

#### 6.1 Academics and policymakers

The unemployment and health debate can now be said to have entered the third stage of a "social-problem process" as seen by Manning and Downs. This chapter will therefore begin by setting the stage for an attempt to break out of the "loop" described in chapter 5, and then show how entrepreneurial activity by officials failed to accomplish this, and how similar activity by the pressure group forced the issue slightly more successfully. In this stage, what is important is not the development of new knowledge claims, but the promotion of value claims more or less regardless of the state of the academic debate, as reflected in the decision to emphasise "policy" rather than "research" in the major event of the year in the unemployment and health debate.

##### 6.1.1 "Mere associations"

During the summer of 1982, one paper appeared putting the other side of the story: Jon Stern's "Does Unemployment Really Kill?" in *New Society*, was published, coincidentally, it seems, the day after the OPCS Longitudinal Study, 10 June (p 421-422). Stern noted (correctly) that up to that time, debates had centered around Brenner's work. He posed the question of why the relationship between unemployment and mortality should be the major issue, and concluded that apart from the obvious headline-worthiness of "Death ...", the finding of high mortality rates in groups of people, whether socially or geographically defined, with high unemployment rates, is so consistent. However, "the statistical association" he insisted, "demonstrates nothing at all about cause". What if the real problem was that the unemployed "are far more likely to live in depressed areas, to have low incomes, and to live in bad housing conditions when they ARE in work?" He succinctly summarizes the "ceteris paribus" question: Given that "a lot of unemployment is concentrated among groups who have high mortality rates anyway, one must also allow for ill-health -- physical or mental -- causing unemployment and/or causing people to remain unemployed for long periods." The reason Brenner's

time-series had obtained results strongly suggesting a causal relationship between unemployment rates and mortality rates for the period 1926–76 was the massive social change which occurred roughly in the middle of this period which included the introduction of the Welfare State in Britain. Stern enumerates specifically the improvement in working class diet and medical technology (though not Beveridge's aim of "full" employment). He concludes, however, with a personal opinion that unemployment "does have some effect on health" though this effect has not yet been satisfactorily demonstrated.

At the end of October, two papers were published which aimed to give an overview of the whole debate, from two rather different perspectives. One was published in the Times Health Supplement under the major heading "Out of Work, Out of Sorts?" (29 October 1982), subheaded "Where are today's pioneers of public health?" The double page spread included two articles by Scott-Samuel and one by Stephen Farrow (the organiser of the Cardiff Conference). There are two interesting aspects to Scott-Samuel's contribution. On the "academic" side, he admits that Brenner's work has not been able to demonstrate causation (as opposed to "mere association"). On the "professional" side, Scott-Samuel draws a blueprint for how unemployment can be defined as the sort of social problem to which Manifesto community medicine may offer itself as the answer:

I have attempted to provide a model for health and local authorities (ideally via their joint consultative committees) to address locally the health problems created by unemployment"

The idea of joint action by health and local authorities is, of course, an appeal to the pre-1974 Public Health role. This comes out even more strongly in the piece by Steven Farrow:

The first MOH in Glasgow ... was in fact dismissed for making too much fuss about housing. If one looks at the modern equivalent, the district medical officer, it is difficult to uncover the same level of concern for the health of the community and passionate desire to alter the system.

The second "overview" paper to appear in October 1982 was "The social consequences of unemployment" by Catherine Hakim, a principal Research Officer at the Department of Employment. (She had previously worked at OPCS, at the same time as Fox, who, it will be remembered (see section 5.2.2.) had previously worked at the HSE, a part of the Department of Employment. So, in a

sense, Fox and Hakim “crossed over”, in organisational terms.)

She comments that:

the very high and rising levels of unemployment currently experienced appear to have been absorbed without widespread breakdown of the social and political orders, although it no doubt contributed to the riots of Brixton and Toxteth in 1981.

One reason, she suggests, is that unemployment, despite the increasing level, is still highly concentrated amongst certain social groups, industries and regions. In other words,

Labour market segmentation means that unemployment is concentrated in the secondary labour market ... there is extensive evidence, ... that a small minority of the labour force accounts for the major part of the total volume of unemployment over a period of years.

Hakim was not the first to raise these questions. For example, Jon Stern’s paper “Who bears the burden of unemployment?” had been published in 1979. What Hakim does do for the first time is to juxtapose (if not exactly relate systematically) the issues of the unequal social distribution of unemployment and the health of the unemployed. But Hakim has a more detailed grasp than any of the epidemiologists of the distribution question, the income replacement question (many people becoming unemployed in the present recession have not enough contributions in the previous relevant year to entitle them to unemployment benefit at all, but must rely on Supplementary Benefit from the beginning of their spell), the standard of living of the unemployed, and the tendency of individual spells of unemployment to take place in a history of intermittent work.

Her review also throws some light on the differences between the perspectives of the medical statisticians/epidemiologists and the economists. This emerges from her comments on the significance of the use of “aggregate data”. This type of study, such as Brenner’s, carried out by economists and using econometric methods, cannot demonstrate causality, and is vulnerable to “ecological fallacy”, she admits. However, their advantage is that they “lend themselves more readily” than individual-level studies to processes of social accounting and cost-benefit analysis. It does not matter, from a policy point of view, she writes, whether a rise in crime accompanying rising unemployment is due to unemployed people themselves committing crimes. Only “targetted” policies need individual-level research. For example, youth employment policies

would be ineffective in preventing crime if it were the unemployed PARENTS who committed crimes.

It might seem strange to a non-economist, or to any researcher not involved in cost-benefit or social accounting that a method which could not establish causality might EVER be preferred over one which could. But Hakim's paper demonstrates the ways in which the manner of their involvement in the policy process appeared to exclude certain forms of "understanding" (or "enrolment", see sections 1.6 and 2.2) between those participants in the debate who did work with concepts of unemployment patterns and the labour market (mostly economists) and the social and medical statisticians (even those who were also working for Government) from whose accounts of the health effects such terms were missing. Because aggregate time-series studies play a part in policy formulation, even critiques of Brenner by economists were followed by ALTERNATIVE aggregate time-series analyses (Such as those of Gravelle, Hutchinson and Stern, Forbes and Macgregor 1984, and Macavinchey 1984). There was no move to attempt to incorporate the concepts discussed in the TEXT of the economists' papers into the individual-level statistical work made possible by the LS data set. It is interesting to speculate whether Hakim could eventually have done this integrative task if unemployment and health had been more than one minor concern amongst many others for someone in her position. However, in mid-1987 Hakim was considering writing an update of the 1982 paper, subject to "time constraints". According to some accounts, there had been, in the mid-1970s, a closer collaboration between government researchers involved in health and demographic issues, and those involved in labour market issues, mediated by the relevance of the Decennial Supplements (carried out by OPCS) to the work of the Health and Safety Executive (located within the Department of Employment)<sup>29</sup>. This relationship had, it was felt "tended to lapse" during the 1980s.

In an important passage of the paper which anticipates developments two to three years away, Hakim asks whether the rising prevalence of unemployment may be expected to "dilute" the (purported) ill-health of the group amongst which spells (particularly long term spells) seem to be

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<sup>29</sup> One example of this was the Women in Employment Survey (Martin and Roberts 1980), commissioned by DE, carried out by OPCS, and co-authored by a member of each department.

concentrated.

In practice the opposite trend is observed,... that a second process is clearly at work, with unemployment itself contributing to an increase in morbidity and, at the extreme, mortality.

This finding is unreferenced, and was derived from as-yet unpublished results of the Longitudinal Study. She is also aware of the relevance of Townsend's work on the close relationship between poverty and instability of employment, so that "attempts to distinguish the effect ... may be primarily an academic exercise." The implication of rising unemployment, whatever the causal chain, she feels, is that there will be "increasing pressure on health services, and associated costs, in a period of, or in areas of, protracted high unemployment."

At the end of November, a Marplan poll carried out for the Guardian showed that "a mood of fatalism" had settled over Britain, whereby seven out of ten voters thought unemployment the dominant political issue, but few thought there was any solution on offer by opposition political parties. Ten times as many people cited unemployment as "the main problem" as cited inflation, with law and order taking second place. This "mood of the country" may be considered one reason why the new research on unemployment and health had so little impact. Other reasons may have been that at this time it was not the policy of the MRC to encourage "information subsidy" by its staff (though by early 1986, under pressure of cuts in research council funding, it appointed a Press Officer), that the LS monograph was too long and complicated to be digestible by the media, and that no pressure group had, as yet, produced anything suitably pre-digested. Fox, and also Cook, were somewhat disappointed by the lack of impact of their work. They had, at that time, little idea of "information subsidy" or of how the media work, and rather expected journalists to contact them enthusiastically, having read the full academic paper or monograph. Only poor scientists, they felt, needed to engage in deliberate "soapbox" exercises.

### **6.1.2 Policy and strategy**

From 1982 onwards, in terms of "public opinion", unemployment came to hold a somewhat paradoxical position. From available evidence, it seems that



unemployment was both acknowledged as the biggest "social problem" in British society, AND that it was *decreasingly* regarded as something which could strike at anyone. For the majority of people, therefore, to be worried about unemployment came to be seen as a disinterested moral concern rather than an immediate worry. These tendencies had been predicted by Daniel (1981) and by Hakim (as discussed in the last section), and were confirmed in a survey carried out in 1986 (Linton 1987). which showed wide contrast in the issues which respondents saw as "important to the nation as a whole" and "important to them as individuals". In terms of labour economics, these shifts in public attitude were well founded upon the tendency of the British labour market to become *more segmented* as unemployment stabilised at a higher level, after the rapid rise of 1981. In terms of debates on the effects of social conditions upon health, these social and economic trends could have prompted a turn away from the effects of unemployment per se, back towards the wider debate on social inequalities. Although it was never explicitly spoken of in this way by researchers, this is in fact what eventually happened. In many ways, the unemployment and health debate was only temporarily detached from the debate on health inequalities.

In terms of the model of the social problem process proposed in Chapter 1, the issue was now caught in a "loop" -- arguments for and against an "effect" becoming increasingly academic and unrelated to plausible or seriously fought proposals for policy change. SCUF was perhaps somewhat demoralised by Jon Stern's address to their September 1981 meeting, which Trevor Davison remembered as "crushing". Nevertheless, Davison and Jennie Popay had been working on an article for the Times Health Supplement with a community medicine academic of the "Manifesto" school, John Ashton. The group hoped to produce an "Action Pack", some speakers' notes, and to circulate a list of people willing to speak on the topic to all interested or potentially interested organisations. However, the first meeting of 1982, on 6 January, was only attended by 5 people, and by March of 1983 this group had stopped meeting altogether, for reasons which participants were not fully able to explain, but seem to have been dominated by pressure of work on Popay and Davison, who both took up new posts during the period.

Early in 1982, pressure groups were picking up some echoes of the debate going on inside the Health Departments as to whether or not further research was needed. On March 8th, SAPU researcher David Fryer told a meeting of the

UHSG that a review paper by Ken Mullen and Raymond Illsley of the Medical Research Council's Medical Sociology Unit in Aberdeen was being circulated around academic departments "with an invitation to submit research projects to SSRC", although it had been decided by SSRC at council level not to go for a major initiative on unemployment and health. Most research councils have traditionally adopted a "re-active" mode of operation, whereby they respond to applications. But at this time SSRC was making a particular effort to "drum up" applications on specific topics, or utilising specific methods (for example, ethnomethodology). Illsley may, in this case, have been circulating suggestions for research proposals on a number of topics as well as unemployment and health. In any event, the UHSG seem not to have taken any particular notice of this piece of information.

In the summer attendance at UHSG meetings was falling off and discussion on the role of the group began to be felt necessary. The July meeting was the first one in which the possibility of publishing a pamphlet or series of pamphlets was discussed. Steve Watkins also raised the possibility of a North Western Unemployment Alliance. Having by now read Cook et. al's paper and the relevant bits of the LS, he told the group that "the causal link between unemployment and health could be confidently upheld, " and felt it was "important that people should realise this, that DHSS and political interests [sic] ... should appreciate that scientific evidence was now available to disprove the claims based on the inadequate and unscientific DHSS Cohort Study."

It is notable that, despite passing references to invitations to tender for research funds extended by Raymond Illsley, the UHSG shows, throughout the period from late 1981 to the end of 1982, no awareness of the most important behind the scenes activity going on at the Elephant and Castle. Had they operated in the way Whitely and Winyard (1983, 1984) describe other types of pressure groups, keeping in close touch with actual or potential sympathisers within relevant government departments, they could hardly have failed to become aware of developments. As it was, the DHSS, perhaps because of the role played by the Cohort Study in the controversy over Brenner's work, was seen as monolithic and hostile. Whiteley and Winyard have stressed the importance to the success of a pressure group of being able to "respond quickly and understand the [Whitehall] environment within which policy is made..." (1987, p. 63). Heclo and Wildavsky have suggested that just being in London can be important for the effectiveness of a pressure group, and it must

be remembered that members of UHSG seldom came to London at this time (a fact that was even commented on by members of SCUF). In early 1981, Scott-Samuel had met Sir George Young's secretary at the Northern Regional CHCs meeting which both he and Young had addressed, and suggested that she write an article for Radical Community Medicine about research in the DHSS. This never came to fruition, however. The only regular contact the Study Group had with the the Elephant and Castle was to send a set of minutes to an official (John Middleton) each month, minutes which were on occasion carefully doctored to ensure that information judged sensitive would not be entrusted to a civil servant.

What they might have discovered (and had in fact been mentioned to Scott-Samuel at the CHC meeting, without his realising its significance) was that a small policy unit called PSU (Policy Strategy Unit) had been set up within the DHSS in 1980, on the model of a mini-think-tank. Middleton was one member of this group. It existed from 1980 to 1984, and in a Minutes of Evidence to the Social Services Select Committee (Social Services Select Committee 1981, p.2) was described thus:

the policy planning unit [of the Community Services Division of the DHSS] has been replaced by the policy strategy unit, headed by an assistant secretary and composed of 3 full-time principals and 3 part-time professional staff. It is charged to maintain an overview of policy work in all parts of the Department, preparing periodic review of policy initiatives and identifying all apparent gaps (particularly of a cross-sector character). It will also receive papers on all major policy reviews within or involving the Department, and is expected to comment from the angle of wider and future policy considerations. Additionally, the unit will carry out specific policy studies or reviews, mainly but not always short-term,...

The Policy Strategy Unit had presumably taken up the issue of unemployment and health precisely because it crossed departmental boundaries and required a wider perspective. Therefore it met Prince's criterion (see section 3.3), in that "the unit [and in this case perhaps ONLY the unit] can do it" (for a discussion of the history of policy planning units, see also Blume 1987, p80-84). PSU closely matches Prince's account of an entrepreneurial policy or research group concerned with its own "organisational survival". It was described to me as "close to ministers .. a top-level special initiatives

unit, very small, full of high-flyers ... very high-profile in its time"<sup>30</sup>.

Jack Barnes, a member of PSU, was present at a meeting in Professor Walter Holland's Department of Community Medicine at St Thomas's Hospital Medical School on 14 October 1981 (also present were Alwyn Smith and Stephen Farrow). It had been called to see what light the combined forces of community medicine and epidemiology might be able to throw on the effect of unemployment on health. Much of the work of the St. Thomas' department is funded by the DHSS, which makes it possible that this meeting was also a PSU initiative. On 14 July 1982, a proposal was submitted to the Health Services Research Committee of the Scottish Home and Health Department by two health economists, Steve Engelman of Edinburgh (who had attended the meeting at St Thomas') and John Forbes of Glasgow, and from there passed on to the DHSS (see section 4.3.3). In accordance with his encouragement of research proposals on unemployment and health from methodologically sophisticated researchers, Raymond Illsley had promised Forbes and Engelman that theirs would "go the rounds", and indeed it had. It ended up in a DHSS file as having been "deferred pending consideration of resources", with an attached proposal for a joint initiative between the Health Departments, the Department of Employment and the research councils.

In September 1982, DHSS officials involved in the work of the PSU submitted a proposal to Ministers encouraging further research. Officials came down on the side of a large-scale longitudinal study, of the type which academics had consistently advised. This preference was also supported on political grounds: senior administrators cautioned that anything less would be seen by MPs as a "low-key response". The Department of Employment was thought to be about to publish a report on "Social and other aspects (including health) of unemployment" which "was not likely to be reassuring". This could be a reference to Hakim's review which was about to be published, or to the DE-funded work on "Long Term Unemployment and Labour Markets" undertaken by the Policy Studies Institute (White 1983). Ministers were warned to expect fireworks in Parliament, and would "want to have a line agreed" on unemployment and health by the next session. The seriousness of the issue

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<sup>30</sup> However, it must also be said that civil servants were critical of the factual accuracy of Prince's account of what went on during this period inside DHSS, and saw his book as more useful "theoretically" than as a practical guide to what "really happened" within the department.

seemed to justify a large, 3-year cohort study, despite the projected cost of £20,000 as "anything else would be hard to justify in the House". Other studies, smaller in scale, might be useful (these officials admitted) to "promote good practice" by health authorities, however, any discussions on job creation might risk "stepping on the toes of the Department of Employment or MSC", something to be strictly avoided.

By early November of 1982, the PSU and the Chief Scientist's Office were already in the position of having to try and change the minds of the unsympathetic Geoffrey Finsberg and Kenneth Clark. "Crudely" one official later remarked, "The Minister didn't want to know." Kenneth Clark had been willing to admit, as he frequently did in public, that unemployment was bad for everything, health included. The answer was, therefore, to create more real jobs, which was what government policy had consistently attempted to do in any case. The civil servants protested that the Health and Personal Social Services side of DHSS had a duty to see whether there were measures it should taken to mitigate the effects of unemployment, and justified their persistent desire for research to proceed on these practical grounds. Different measures might be needed according to whether ill health was a cause or consequence of unemployment, and only the long-term study of the problem could answer that question. Their appeals were unsuccessful.

On 17 January 1983, Mr. Ernie Ross, MP for Dundee (West), asked the Secretary of State for Scotland "what evidence he has that infant mortality rates amongst unemployed families are greater than amongst employed families", and "if the Secretary of State was sponsoring any research projects into the effects of unemployment upon health in Scotland" (Hansard 25 January WA col. 37) Under-Secretary of State for Scotland John Mackay replied "My department is not funding any research projects directly related to the effects of unemployment upon health in Scotland". On 25 January, Ross followed-up by demands that the Secretary of State for Scotland seek to fund research projects related to the effects of unemployment upon health in Scotland. (Hansard 25 January WA col 423). At this time, Under-Secretary of State Mackay responded "My Department has under consideration an application for funding of a research project related to unemployment and health." What may have happened here is that after the decision of November 1982 not to provide DHSS funds for a project based on Forbes and Engelman's proposal, the Scottish Home and Health Department gave some consideration to funding a

more limited project. No one I spoke to in Scotland in the spring and summer of 1983 mentioned any further applications subsequent to the rejection of Forbes and Engelman's. However after the decision of November 1982, DHSS funding looked extremely unlikely. Forbes remembers getting a letter finally turning down the proposal "sometime after the general election". In the event, Mackay's answer seems to have satisfied the Scottish MPs for the time being, as the Adjournment Debate on Unemployment in Scotland introduced by John Maxton on 8 February contains no references to health at all.

None of this was reflected in the recorded discussion of the members of the Unemployment and Health Study Group, either at the end of 1982, throughout 1983, nor indeed as late as 1986. Steve Watkins commented that UHSG had not really been set up as a "pressure group" at all, but more as what its title implies -- a study group -- to support his own research, undertaken as part of his training in community medicine, and that of others such as Birt and Westcott. In 1982, he felt, looking back, the group's meetings had become "stereotyped meetings that had lost their point." It had kept going partly because there was a demand for information and speakers (as SCUF had also found), and because his own work and that of Westcott still needed a forum for discussion. The group's only new idea was on the "politics of unemployment", and Watkins worked some of this into the paper he gave to another WHO conference, "Health Policy Implications of Unemployment", held in Leeds in December of 1982<sup>31</sup>. Although the conference, and the papers presented there, had very little impact on the wider public debate and none whatever on the "scientific" debate, it began to turn the ideas of the Study Group away from research and towards "policy". The first result of this was the attempt to set up a North Western Unemployment Alliance (see section 6.1.2).

## 6.2 Breaking the loop

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<sup>31</sup> Proceedings of this conference have appeared as Westcott et. al. (eds), 1985.

### 6.2.1 Interests and alliances

Early in 1983, Fox and Scott-Samuel entered into a discussion on the question of the fragmentation of the issue of health inequalities. The occasion for this was provided by a series of three seminars on health inequalities in Europe, financed by the ESRC and the European Science Foundation. Fox, as the organiser, invited Scott-Samuel to participate (no other member of the British medical profession was invited). They were both disturbed by a recent paper by one of the government economists involved in the unemployment and health debate, which showed by means of a mathematical model that it was theoretically possible for most of the inequality in health between the social classes in Britain to be attributed to downward social mobility of those with adverse prior characteristics (Stern 1983). Both in tone and content, the communication between statistician and "Manifesto" community physician carries striking echoes of Mackenzie's account of the way in which professional involvement in public health affected the development of intellectual currents within medical statistics in the early twentieth century. Genetic determinism (one sense in which "prior characteristics" could be read) would have left no space for a reformist practice of public health, as Major Greenwood had concluded (see Mackenzie 1981, p.111), nor for a statistical practice oriented towards public health as its client profession. In his comments on Stern's paper, Fox was concerned that the explanation of inequalities as being due to selection and selection alone would be the most acceptable to government, as it would give inequality the appearance of being "one of the laws of nature and therefore beyond our control."

This may have been something of a misunderstanding on the part of those academics who were now beginning to form links with "Manifesto" community medicine. Fox saw Stern as motivated by "political interest". The last few paragraphs of the paper by Gravelle, Hutchinson and Stern, as well as the authors' interviews and other publications, imply that they, like the "social statisticians", took a broadly reformist, "social engineering" approach to policy questions. It was no more in their interests than in those of "Manifesto" community medicine to see ALL social problems as "natural". The differences lay rather in WHICH problems could be seen that way. Health economists had little interest in developing (or re-capturing) a more general political-advisory role, similar to that of the MOsH, centered around the physical or

socio-political "environment". Health (and other) economists were well entrenched within the existing structure of government departments (see section 3.2), nor was their role in any way threatened by the spectre of health service reorganisations<sup>32</sup>. As far as the role of economists who acted as advisers to the Health and Personal Social Services side of the DHSS was concerned, they could afford to accept that one of the things government should do is deal humanely with some sort of small "residuum" or "underclass", a phenomenon which both Stern's and Gravelle's published work seems to imply. If social class mortality differentials could be shown to have widened during the existence of the NHS, the economists may have felt, this "fact" could be used to play into the hands of those who wished to challenge the very concept of existing welfare measures such as a free health service, social security and so on<sup>33</sup>. Now, this DID threaten their role. The disagreement between the economists and Manifesto community medicine and its academic allies was frequently couched by *participants themselves* in terms of the political allegiances of their opponents. However, analysis of the progress of the debate over time seems to point to a greater influence of professional (including "personal advancement") rather than political factors. Positions adopted in relation to the "facts" seem to have been the result of different perceptions of potential tasks, alliances, and balances of forces amongst the entrepreneurial and innovative members of professional and disciplinary subgroups. But it should not be assumed by the coincidence of abstract ideas and professional interests that the relationship between the UHSG, particularly its "Manifesto" community physicians, and academic social scientists and statisticians were simple or straightforward. The correspondence between Fox and Scott-Samuel (which was extended into a working relationship that lasted throughout the present debate and into another) demonstrates ONE form of interaction between members of different subdisciplines and their professional "clients".

Following the social mobility paper in February 1983, another paper by Jon Stern was published in *Population Studies* in March (Stern 1983), which was on

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<sup>32</sup> The abolition of the Area tier of Health Authorities in England in 1982 had done away with some Community Physician posts, and the Griffiths proposals for General Managers threatened the positions of these "clinical managers" even more, see Ham 1986.

<sup>33</sup> For an explicit statement of this position see Illsley 1987, Illsley and le Grand 1986, Le Grand 1987.



unemployment and health. The paper did little more than summarize his existing work available in the Lancet and in two Discussion Papers from the Centre for Labour Economics which had been available since late 1981. It contained some very technical discussion of the use of panel data, including that of the National Training Survey, on which Stern's ex-colleagues at CLE, Steve Nickell, Wiji Narendranathan, and David Metcalfe had been working. This longitudinal study of a large sample of 17707 men aged 25-65 in 1975-9 was interpreted by Stern as showing that experience of sickness before 1965 increased the probability of both sickness AND unemployment after 1965. But a spell of unemployment before 1965 did not increase the probability of a spell of certified sickness after 1965. Unemployment between 1965 and 1975, however, did increase the probability of a long spell of sickness during the same period, and conversely. But the data do not allow a causal order to be established. Stern concluded that

inequalities in mortality and other health indicators, and inequalities in the incidence of unemployment are jointly caused by wider inequalities of income and wealth, education, and inherited characteristics. If our concern is to improve the health of people with low incomes ... then the effect of unemployment on health is largely a red herring.

Perhaps because it was essentially no more than a recapitulation, this paper seems to have been less quoted (formally and informally) in the subsequent debate than the "mobility" paper.

In the April edition of the Journal of the Royal College of Physicians, a review paper by Steven Farrow provided an up to date statement of what the "state of the art" was thought to be in the debate.

Unemployment [he states] presents a difficult problem for the researcher because of its interrelationship with other important social factors, including class income and housing. In addition, the so-called healthy worker effect has to be distinguished so that the specific effect of unemployment on health can be examined.

Farrow is therefore the first academic to see the importance of the concept of a "healthy worker effect" as it was used in the work of the LS team (see section 5.2.2) to the unemployment and health debate, although he does not go into any more detail, and to most readers this may have meant very little as it stood. "Almost nothing" he complains, "is known about the community's health

at a local level", and bemoans the lack of a clear role for community medicine. In a statement of the Manifesto position, (though Farrow never attended UHSG meetings and did not have regular contact with the community physician members of the group), he concludes:

The evidence linking unemployment to health is circumstantial but strong. In these circumstances, medicine ... has a duty to speak out against those policies that lead to increased unemployment.

An example of a group *altering* its strategy to pursue a new set of alliances, and of the way in which this strategy affected the group's assessment of knowledge-claims, is given by the next phase of the UHSG's development. At its first meeting of 1983, congruent with the general tendency throughout the debate for labour movement organisations to be uninterested in "the health of the unemployed", it was reported that the initiative to set up a North-Western Alliance had met with a very disappointing response -- only two County Councils and a single Trade Union had replied. The February date for the first meeting was postponed. So UHSG decided to take over the organisation of what was at first described as a "national research conference". This might seem at first to be a turn away from "politics" towards "science". But from the beginning, the purpose of this conference was ambiguous. Steve Watkins remembered it as having been projected as a "policy conference", following-on the WHO meeting in Leeds in December 1982. The minutes give a different picture. This ambiguity was to be reflected in the final outcome. The quality of the MRC-funded work published during 1982 was generally acknowledged by UHSG, but apart from Steve Platt, none of the authors had been invited to the Leeds WHO conference -- the "policy" theme having predominated. The Regional Heart Study researchers were committed to extreme caution, and Fox, at this time, was (at least publicly) far from convinced that the LS results were not due to prior selection of the unemployed by ill-health.

At the 24 February meeting of UHSG, further discussion took place on the desirability of holding a conference, but no decision was taken as to whether it should be on "policy" or on "research". Members of the Study Group were usually prepared to be very bold in making claims about the "evidence" that unemployment caused ill health, but seem to have hesitated over the question of whether further discussion of "the evidence" could be done away with altogether. It seems that even by those sympathetically inclined, the debate on

whether or not unemployment could be regarded as “causing” ill-health was not at this time regarded as “settled”. The appearance of new, post-Brenner British research using less contentious methods had not made that much difference to the position which any person or group wishing to be seen as “fair” and “reasoned”, whatever their sympathies, could take.

Of course, while all this was going on, a general election campaign was being waged. The involvement of various members of the Study Group in the Labour party has been discussed in section 4.1.3. There is no suggestion, either in UHSG minutes or in interview accounts of what was happening to the group at this time, that the election affected the decisions it took or the plans it made. However, the presence of only three people at the meeting on 21 April may have been partly due to members’ other political commitments. At this meeting the idea of a North-Western Unemployment Alliance was decisively dropped. Most time seems to have been devoted to discussion of the proposed conference, or as it now comes to be called, Workshop. It was decided to hold it in Leeds in late November, and to deal solely with policy. Afterwards, a smaller group of 12–15 people could stay overnight at the Nuffield Centre for Health Service Studies and write a document which the Centre had agreed to produce as an Occasional Paper. Research papers, were to be solicited, but only as “background”. By 6 July, the Study Group had decided (at a meeting of still only three people) that precirculated research papers would only prove “diversionary”. So brief “summary papers” were to be requested, from Watkins on physical health, Platt on suicide and parasuicide, and Prof Peter Warr of SAPU on general mental health effects.

### **6.2.2 “Making the papers”**

Some of the ambivalence of the atmosphere in which the debate now proceeded, and the sheer contingency of some of the processes involved, may be seen by close examination of the way in which unemployment and parasuicide made its next major appearance in the media. On 13 March, Steve Platt gave a talk on youth and drugs at Moray House College of Education in Edinburgh. A Scotsman reporter, Bryan Christie “covered” the event as part of his routine work -- tasks are allocated to reporters at the beginning of a working shift, and this one happened to fall to him. He reported it on 14 March, but this was not the end of the matter. As it happened, Christie had ambitions

to be a feature writer, was interested in health issues, and the Scotsman had no-one else writing on health at this time. As Christie described events:

It was just an off chance. He [Platt] said he hadn't finished the research yet, so I left it for a while, and then phoned him and said how's it going? He said he was writing it up so I left it a few more weeks. I was looking for things to write features about. ... The trouble is, if you follow up something like this, you still have to do all the other stuff as well, all the routine reporting.

Nevertheless, Steve Platt felt that Christie dealt with the issue (in a feature which appeared on 2 August 1983) with great care and accuracy. The article acknowledges that a "significant relationship" does not amount to a "causal link". However, Christie quotes Platt:

"The trouble with this area of research is that it is very contentious and the academic community have in the past sat on the fence. They have felt for too long that they have to dot the i's and cross the t's but it can now be said that the risks associated with long-term unemployment are just not acceptable."

Combining the requirements of "journalist" with those of "science writer", Christie added other items to his summary of the work on parasuicide: a "human-interest story" about two young men who suffocated themselves, leaving a note saying they had "nothing to live for"; parliamentary questions, including the one from Frank Allaun, and Ministerial denials.

In common with other Scottish journalists whom I interviewed, Christie bemoaned the way in which the organisation of work and limitation of resources on a small newspaper made it difficult to deal with issues in depth. As he put it:

I can understand that the journalistic profession is not renowned for treating things like this with care. It is very easy to read the figures wrong. If you're handed something on a piece of paper at five thirty and told you've got half an hour to work on it, mistakes do happen.

and his colleague on The Scotsman, Robbie Dinwoodie, remarked:

You have to understand ... there are only nine reporters on The Scotsman. So ... journalism becomes a reactive business. If someone doesn't come to tell you about something [that happens in a scientific debate], you'll never find out about it. It's fire-brigade journalism.

To add to the difficulties, Christie had other strong interests apart from health at this time, for example, in defence issues. Sarah Nelson, who also wrote some features on health and medicine, was soon offered (and accepted) the specialised post of education correspondent, a particularly important topic in Scotland. Not only for unemployment and health, but for any technically complicated issue, lack of a specialist writer was a serious problem. In the case of "The Scotsman", quality did not suffer, but quantity may have.

Christie concluded his 2 August feature in eloquent Calvinist vein:

Work, it has been said, is nature's physician ...

He admitted later, however, that there had been no letters to the newspaper following his feature, and that he had been "surprised at how quickly it went flat". By the time Steve Platt felt ready to discuss his work, Parliament had gone into recess, so that it had been

hard to find people to chase up for comments on it. It was hard enough work trying to make sure I got all the figures right .. I phoned the Press Office at the SHHD and tried to arrange a meeting with MacKay. Of course he wasn't going to have any of that.

The World at One did follow up the article, but no other Scottish newspaper did, not even the Edinburgh Evening News, which shares premises with The Scotsman and has a good local reputation for covering medicine and health. On 4 August the Daily Telegraph carried a very small piece and the Morning Star gave the story rather more space ("Joblessness Kills, Shock Probe Reveals").

At around the same time, the regular bulletin reporting the doings of MRC researchers, "MRC News" carried a long discussion of Platt's work, and this was reported in the Daily Star as:

Doctors are shocked by a huge increase in suicide attempts by the unemployed ...

the Medical Research Council says there is "cause for alarm"

This somewhat sensational-sounding phrase, "cause for alarm" is in fact a direct quote from MRC Bulletin No 20. It provides an example of the fine line to be trodden by the scientific entrepreneur, between keeping a social problem

sufficiently in the limelight and not giving grist to the mill of sensationalism. The report in MRC News cautions that the existence of the association demonstrated by Platt's work does not "constitute proof of a causal link," and then goes on to state that "a causal explanation appears more likely".

In November of 1983 Steve Platt's research on unemployment and parasuicide was published in the Unemployment Unit Bulletin. Like the report of his work in "MRC News", this paper stresses the "public health" aspect of the problem:

Clearly, far more research remains to be carried out in this area. It should have high priority ... because of its practical, public health implications. If trends in long-term unemployment are indeed crucial for predicting the future course of parasuicide, then there is grave cause for alarm...

This was the "social problem claim", and was followed by the "factual claim" that:

Whatever the nature of the association between unemployment and parasuicide, it has been shown that [the] long-term jobless currently run more than 18 times the risk of parasuicide of the employed.

and finally, the proposed solution:

This .. risk may be reduced to some extent by allocating more resources to help alleviate the economic, psychological and social impact of prolonged joblessness ... But ...these measures do not address the fundamental underlying problem ... Urgent government action is required to reduce the level of unemployment (Unemployment Unit Bulletin, November 1983, no 10 p 4-5)

Surprisingly, the Scotsman carried no report of this publication. The Unemployment Unit was experienced in the art of information subsidy as practised in England, and had provided copies of the Unemployment Unit Bulletin to all national newspapers' head offices. Many did carry the story. The editor of The Scotsman decided not to carry another article as it was too soon after the August one. Bryan Christie was on holiday. When he returned he was dismayed at his editor's decision. To add insult to injury, the Glasgow Herald HAD covered the story. As he put it:

Not many people read both the Glasgow Herald and the Scotsman, except for journalists, that is! But if they had, it would

have looked as if the Herald had got a good story and we had missed it altogether. At the very least, we could have pointed out that we covered it months ago ... We could have pointed out that we had beaten the Glasgow Herald.

In fact, the article received wider coverage in English newspapers than in Scottish ones, which puzzled Steve Platt. This was a result of the Unemployment Unit's unfamiliarity with the ways in which the media deal with Scottish issues. The Daily Mail contacted Platt from London after reading the story in the Glasgow Herald. Why was this necessary when copies of the Bulletin had been sent to the Mail's London HQ? The right thing to have done, explained Bryan Christie, was to have sent it to the Mail's *Scottish* HQ. Otherwise, if they decided it was not a "British" issue, London would not bother to notify the Mail's Scottish offices in Glasgow. If one had wanted to get coverage in the Scottish Daily Express, just to complicate matters further, the best thing would be to send press notices to the Express's Manchester offices, where *their* Scottish News Desk is located. "It all depends," he pointed out, "what you mean by a *national* newspaper."

This account of how one piece of research "made the papers" shows the operation of an "accident-prone process" by which scientific "findings" reach the wider public (as shown in section 4.1.2. for World in Action's coverage of Brenner's claims). It also provides an example of how a researcher's participation in one social-problem process (on drugs) can lead to the adoption of an "expert" role in quite a different one. This account demonstrates the role of "chance" in giving public importance to a researcher's work. But it also shows the part played by social-problem claims-making in the cycles of credibility of *both* academics *and* journalists. In the collaboration between Platt and Christie, appears a type of symbiosis similar to that between officials and activists discussed by Richardson and Jordan (1979, see section 1.2). Like the official, the journalist is only "interested" in a piece of research as it relates to one topic on a long list s/he has to deal with. But, like the pressure-group member, the researcher has specialist knowledge which may benefit the journalist in two ways: both by helping him or her to write something "new" and "interesting", and by *subsidizing* complex information, effectitvely reducing the amount of time needed to produce an article on a difficult or controversial topic.

**6.2.3 Research to policy: an attempted transition.**

The fairly extensive media coverage of Steve Platt’s paper set an appropriate background for the forthcoming Policy Workshop of the UHSG, which took place on the 24th of the same month. At this point in the unemployment and health Debate, “central” non-academic participants saw this conference as the most important event of 1983.

The first part of the day was taken up with an uneasy discussion which hovered constantly on the brink of the discouraged re-evaluation of “research”. Steve Watkins, author of a position-paper circulated prior to the conference which made very sweeping claims in favour of a “pro-Brenner” position, at one point admitted that “It is still not clear just what it is that is important for health about work.” And Scott-Samuel reminded the meeting that “A minister in the House of Commons can get up and say there are no proven links. They are still able to do this, though I don’t know how they can after the evidence of the Longitudinal Study”.

Composition of the Leeds Policy Seminar

"Activists" (local councillors, trade unionists, local action groups	12
"Academics"	17
of which: Economists	2
Psychologists	4
Sociologists	11
Community physicians	6
GPs	1
Local authority employees	3
Health Educators	6
Total	45

The scope of possible alliances for Manifesto community medicine is visible here: academics from various disciplines, health educators, and “activists” of



various kinds, all groups which were also represented by papers published in the "official" journal *Community Medicine* as well as its "Radical" doppleganger. Some examples can be taken from the day's field notes to try and illustrate the different orientations of these participants. For example, as one would expect, community physicians were concerned about health planning and what the relationship between unemployment and health meant for that aspect of their function, but they were also concerned with their relationship to local authorities and to national policy makers:

What are our policy objectives? My own are to formulate policy to improve health. The objective is not to discuss ways of finding more jobs.

If we are thinking about treatment, then we need a medical model of research. It will need to have credibility with doctors. If you are looking to aim at politicians, other models might be more appropriate. Or if you are aiming at encouraging community action, you could use models which look at the impact of unemployment on whole communities.

One community physician offered an interesting insight into the ways in which scientists "need" there to be different kinds of "problems", in addition to those to which practitioners and planners offer THEIR skills as the "answer":

Those involved in research find it difficult to at the same time be questioning about the limits of the state of knowledge and also to say, in a policy-making sense, 'we can regard this as fact'.

Here is evident the irony involved in the scientists' need to promote a certain degree of "uncertainty", in the absence of which the importance of their own role diminishes<sup>34</sup>. Yet at the same time, researchers must produce enough "facts" to maintain the legitimacy of their claims both to technical effectiveness and to the attention of both public and policy makers (for a recent discussion of this dilemma see Jasanoff 1987).

Academic social scientists (leaving aside psychologists for the moment), did not confine themselves to research and its application, but also made more openly political contributions. However, in general their interventions in the discussion reflected concerns with their relationship to the state:

It is increasingly obvious to me that the response of the state is ... always an attempt to refute these studies [such as Brenner and

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<sup>34</sup> For discussion of this point within one perspective of SSK, see Whitley (1982).

the LS]. That's the way it was in the 1930s and that's the way it is now.

and the media:

Part of the problem is in the timidity of researchers about their own results ... We cannot PROVE the connection in the same way as a randomized controlled trial can. You have to educate people in the media that this kind of research is not 'inferior' to natural-scientific research ...

and with making sure that their own special topic was not left out of the formulation of the "social problem":

The research we have is sexist, racist and individualist, let's not forget that. There is also a total neglect of poverty.

Community workers and "activists"(delegates from Labour Parties, Trade Union research departments, Action Groups, etc) debated the relative merits of attempting to change national macro-economic policy versus those of encouraging local initiatives, though on the whole these participants were not particularly vocal, and the solution to the central-local conflict was offered by an academic who was also involved in political initiatives at both levels:

there seem to be two perspectives among health workers: The first is, we don't want to get involved in unemployment... it's too political" Others take the attitude that the response is ameliorative ,not political enough. One way to meet these objections is not to separate prevention and modification of effects. If people get involved in preventive work, it may help to transform their attitude to unemployment on the wider political level.

There were two groups who contributed fairly extensively to the general debate in this meeting, the psychologists and what I will call the "futurolologists", that is, economists and other social scientists engaged in the study of "long-range" economic and scientific policy and planning. One of the latter, from the Institute of Economics and Statistics at Oxford University, took a pessimistic view:

My research is on what employment will look like in the 1990s. ... The trends in that period [since 1930] are towards increasing output at the expense of shedding labour ... The net increase in jobs ... was in the service sector ... But can service industries continue to grow in the next thirty year period?

Even in the case of some service industries such as banking, he pointed

out, new technology was now cutting jobs. Another "futurologist", Ian Miles of Sussex University's Science Policy Research Unit, felt that as a result, efforts should be turned towards finding other ways of providing the health-protective features of work:

We need to talk about restructuring society so as to provide more opportunities for people to exercise control over their everyday lives, to become involved in social networks, and constructive activities.

The contribution of the psychologists was perhaps most sobering of all. They accepted the prediction of the "futurologists" and saw the task of local educators and service providers to be one of countering the "work ethic". Steve McKenna of SAPU pointed out that 20% of people in their studies experienced improvements in physical health after redundancies or during periods of lay-off from heavy jobs such as shipbuilding. He also felt that "The will to work is diminishing. We should think about how we are training schoolkids to demand work. We're turning out people with a high employment commitment which cannot be satisfied." And clinical psychologist Graham Stokes asked

When we talk about local campaigns, what is our focus? Should we not acknowledge that technology is changing and a lot of people will never get paid employment again? Even if we got back to the level of production of the 1970s, in the West Midlands, the effect on employment would be minimal, because of changes in technology and higher productivity

This account of the meeting which concluded the activity around unemployment and health in 1983 shows the ways in which the composition of groups of discussants profoundly affected perceptions of the very nature of the "social problem". Professional and institutional affiliations not only influenced what was regarded as "fact", or as a "researchable question", but also the ways in which these were, in turn, related to policy prescriptions. Because of the contingencies of social and professional network organisation, the Leeds meeting, which was explicitly about "policy", did not have access to some of the unpublished and proposed research which might have been most relevant to the type of local, grass-roots strategies which many participants wished to discuss (much of which was first publicly discussed at the BSA Annual Conference in the spring of 1984, but which must have been "available knowledge" on the industrial sociology networks by this time, for examples see

Roberts et. al eds. 1985, Wood et. al eds 1987) By accepting as real a series of research findings based on a black-box model of "cause=unemployment" leading somehow to "effect=ill health", participants in the Leeds seminar of November 1983 were unable to deal with questions of local labour market change, as only national and regional trends were familiar to the futurologists. Westcott's research on Scunthorpe could have provided such information. But her perspective, derived from medical and macro-economic models, had not sensitised her to look for it. At the Leeds meeting, it fell to the psychologists to step in to the space provided by the black-boxing of the intervening processes linking economic change to health status. The product of this meeting was the document "Unemployment, Health and Social Policy", the impact of which is further discussed in sections 7.3 and 8.1.1.

This chapter has given an account of the "third stage" of the social-problem process in which those who made the original claims that the health of the unemployed was a "problem", and one to which their skills could be seen as "the answer", rejected the official response. After some hesitation and false starts, this rejection took the form of a strengthening and extension of the "Manifesto" community medicine group's alliances, not with labour movement organisations or political parties, but with other subprofessional groups who also had an "interest" in claiming an expert role. By the time of the Leeds conference, alliances between "Manifesto" community medicine and *academic* disciplines were still at a preliminary stage, which was reflected in the emphasis placed on "policy" and the reluctance to discuss "research". In the next chapter, the consequences of a strengthening of this latter alliance, that between community physicians and academics in medical statistics and sociology, will be traced. A revival of the "moral" claims was followed by a strong revival of "knowledge" claims as well.

**CHAPTER 7**  
**STAGE THREE (B): REASSERTION OF**  
**KNOWLEDGE CLAIMS**

**7.1 January 1984: The status of the debate.**

The last chapter set out the ways in which the pressure group, and some other participants, decided to take the initiative regardless of whether or not the academic debate need be regarded as "settled". This chapter will describe new initiatives by "experts". At the end of the chapter, we will see how these two trends came together and produced a moderate political impact.

1984 began with the swansong of the Unit for the Study of Health Policy. Its original Kings Fund money had run from 1975–1980, and was then replaced by a grant from the Health Education Council, and others for specific projects from the Leverhume Foundation (1980–1983) and the Rowntree Trust (1981–March 1984). By the end of 1983, most USHP staff were looking for or had found other jobs. A final conference was planned for 6 January 1984, which was well attended (both in terms of numbers and the prestige of participants), and addressed by, amongst others, Dr Leo Kaprio of the WHO, and Christopher Huhne, Economics correspondent of The Guardian. All papers were invited. Chris Birt (a member of the UHSG) gave one billed as "Unemployment and Health" which in fact dealt more with the Black Report. The conference was attended by many representatives of Community Medicine, the groups organised around such ideas as "Community Development in health", and "Green economics". Less well represented were medical sociology or social policy.

On 19 January, New Society feature writer Martyn Harris summed up the "state of the art" in unemployment and health research as far as non-expert but informed opinion may have seen it at this time (Harris 1984). Harris reminds his readers that while the average two-child family spent £124.75 per week excluding housing costs in 1981, the current (1983) rate of Supplementary Benefit for such a family was £59.20 per week, around 40% of the amount SPENT (not EARNED) by a family with an employed breadwinner. Under a subheading "The problem of proof" he deals with the debate on health. Work by Len Fagin and Dennis Marsden is described as not "particularly rigorous or

exhaustive". He regards Brenner's work as "The most powerful argument for a causal link between unemployment and health" and reports that although Brenner's results "not surprisingly ... have been savagely attacked" nevertheless "they appear to be standing up fairly well, with many social scientists paying respectful attention to his findings, if only because they don't understand how he obtained them." On mental health, Harris reports, there is agreement, except for suicide and attempted suicide, and here he quotes Steve Platt's work, which demonstrates Platt and the Unemployment Unit's success in producing the November 1983 information subsidy. Although he either has not read or has not understood the LS and BRHS papers, Harris HAS read Catherine Hakim's paper in the Journal of Social Policy. What he picks up from this paper is Hakim's comment on the relative "lack of interest" in unemployment and health in comparison to the 1930s. In his final paragraph he asks:

Is it that the social science establishment has actually collapsed under the steady sniping from the Keith Joseph quarter and lacks the self-confidence to tackle the major social issue of the day? ... It is a bit difficult to argue for the defence of benefits and services if you can't even establish the true COSTS of unemployment to the hatchet men in Whitehall.

In this final paragraph, Harris foreshadows the way in which the media, at least, will now begin to deal with the unemployment and health issue. No longer as a "social problem" in its own right, but as JUST ONE indicator of two other "social problems", now more interesting and relevant to the media, and, importantly, shared by media workers, government scientific advisers, and other academics and scientists. One of these new "problems" was the decline in the volume, availability and intelligibility of officially produced and available information on "social indicators". This was not only a problem for journalists, (who, as has been explained in Chapter 1, need information not just in ANY form, but preferably in a "subsidized" form), but also for research and policy units WITHIN government departments, in the wake of the Rayner reviews and changes in the organisation and funding of official surveys<sup>35</sup>.

The second "problem" was the "decline in British science". For participants in the unemployment and health debate, this came closest to home in the form

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<sup>35</sup> The numbers in the Government Statistical Service had been reduced from 263 in May 1979 to 193 in March 1984, see Boreham (1984).

of the review of the working of the Social Science Research Council initiated by Sir Keith Joseph, as reflected in the above quote from Martyn Harris' article. The review was widely seen as a prelude to cuts in funding for social science. In the event, the recommendations of Lord Rothschild's report (Rothschild 1982) were far more benign (Posner 1982), although in its aftermath the Council lost the word "science" from its name<sup>36</sup>. A new alliance then began to take shape, between officials and government researchers on the one hand, and journalists on the "quality" papers on the other. Academic advisers to government as well as civil servants appear to have been increasingly willing to talk off the record to journalists. In early 1986 a senior academic gave the opinion that:

the really interesting story [concerning unemployment and health in Britain] is why the research in Britain IS so thin ... [but] this could not be investigated by an academic, but would have to be left to investigative journalism. If you tell a journalist something, the more confidential you say it is, the more certain he is to print it.... But as an academic, you [the interviewer] cannot betray those confidences. After all, these people [academics] have to go on trying to get funded by this blessed government. It is a question of the best being the enemy of the good.

In this quote, the speaker also gives *his* perception of how the debate might have been affected by "political context", in that he clearly believes research funds would have been easier to come by under a government of a different complexion. It was in this context that the results of the analyzing the 10-year mortality data from the OPCS Longitudinal Study first became available, and began to be presented as fresh support for the claims that "unemployment harms health".

## 7.2 Point of Passage

The first full account of the ten-year follow-up results of the Longitudinal Study were given in a talk by David Jones, a senior research fellow working with John Fox at the Social Statistics Research Unit at City University, to a conference of the Institute of Statisticians at Kent University in July 1984. One

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<sup>36</sup> The preservation of funding for the social sciences by way of increasing the emphasis symbolically laid upon economics would constitute a topic for a separate research project in the sociology of knowledge, see Flather (1987), which cannot be dealt with here. However, the symbolic change is altogether consistent with the argument of the present study.

half day of the conference was devoted to the topic of unemployment and health. The rest of the conference, on "health statistics" was, however, dominated by topics of interest either to planners or drug companies. Perhaps as a result of this, the session on unemployment was poorly attended.

Derek Cook gave the first paper, in effect repeating most of the material in a review paper which he and Shaper had written for an edited collection of "Recent Advances in Occupational Health" (Cook 1985, Cook and Shaper 1985). For the purposes of this public presentation, however, he emphasised the dramatic nature of the public debate, starting with a slide which showed a collage of headlines representing both "sides" of the debate: "Death on the Dole", "A scandal on the conscience of Margaret Thatcher", "Dole is Like a Holiday, Says Economist". He also reminded his audience that

The funding of research is a political matter. It is futile to pretend that we can deal with such a topic in a purely objective way.

In this and other ways, the tone of this paper was very different from that of the review. Cook laid more emphasis on the material on nutrition, taken from papers by Cole, Donnet and Stanfield (published in 1983) and by Doyle and co-workers (published in 1982). Cole et. al. had studied the birthweight and growth of babies in two areas of Glasgow, one prosperous, the other deprived. They had demonstrated lower birthweight in babies whose fathers were unemployed. Doyle et. al had studied the diets of pregnant women in two similarly contrasting areas of London. Using the painstaking "one-week weighed survey" method, they found that pregnant women in the poorer area were consuming under 1700 calories per day on average, and that these diets were particularly lacking in essential fatty acids necessary for cell-growth, and thus particularly important in pregnancy. That neither of these studies had made any great impact on the public debate was regarded as justified by Cook, in the light of their small size and the fact that they had not been designed to investigate unemployment.

He concluded:

as in the 1930s, national indicators are reassuring, but local studies may tell a different story. I say "may", because I don't think the studies are good enough at present.

However, Cook made no concessions to Brenner, devoting the last section of his talk to an exposition of Gravelle, Stern and Hutchinson's critique ("On the



whole, I feel they did succeed in taking his paper apart"...). In general, he felt, longitudinal aggregate studies of this nature could give us no more help in deciding such issues as whether unemployment affected health.

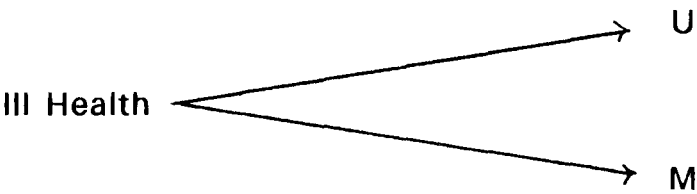
The second speaker, Paul Jackson, concentrated on the psychological effects of unemployment, and will therefore not be dealt with at length here. He did, however, feel that at this time there WAS "more of a consensus on the CONSEQUENCES of unemployment".

Although the first author of Working Paper No. 18 from the City University's Social Statistics Research Unit (SSRU), and of all subsequent papers on unemployment and health was Kath Moser, a Research Fellow in the Unit, she disliked giving papers publicly, and as a result David Jones was allocated this task. The way in which his talk was constructed was typical of LS "style". He began by setting out three alternative hypotheses about the relationship between unemployment and health:

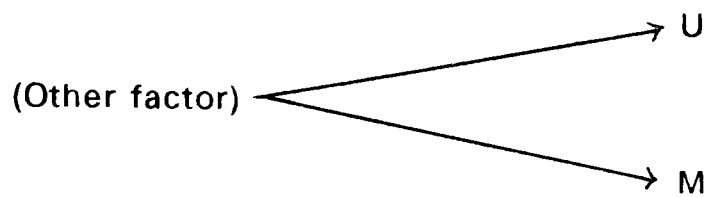
- 1. that unemployment has an adverse effect on health

$$U \text{ ---} \rightarrow H$$

- 2. that unhealthy people are either
  - a. more likely to be unemployed or
  - b. at a higher risk of death [so that it is their ill health which "causes" BOTH the unemployment AND the mortality.]



- 3. That there are OTHER factors associated with both unemployment and an increased risk of mortality ("These might be social disadvantage" he asserted rather vaguely)



(Where: U= unemployment and M= mortality)

He gave the basic numbers on which the findings are based, and here it is notable that the numbers of men "seeking work" in the week before Census Night 1971 who died between 1971 and 1981 were only 328.

	At risk 1971	Died 1971-81	Crude death rate
All men	250,588	29,923	11.9%
15-65	161,699	8,061	5.0%
seeking work	5,861	328	5.6%

(i.e., those "seeking work" were approximately 12% more likely to die in the following ten year period than all men in the sample aged 15-65)

The unemployment rate was 3.6% in 1971

In longitudinal studies, death rates are not calculated in the "crude" manner used in the above table, but by a somewhat more complex method which makes use of the concept of "person-years at risk". Having carried out this correction, the Standardised Mortality Ratio for those seeking work in 1971 was, according to age:

## Standardised Mortality Ratio by Age

Men "seeking work" 1971

	O/E (average=100)	95% confidence limits
15-24	169	ALL
25-34	178	
35-44	201	VERY
45-54	173	
55-64	111	LARGE

"It seems" Jones observed here "that the effect is greater in the young." He then presented the first of the arguments, that the mortality excess of 36% in the "unemployed" was due to their social class distribution:

### Social Class Distribution of the "Seeking Work"

	% Seeking Work					
Social Class	I	II	IIIIn	IIIIm	IV	V
Ut. rate	1.2	1.8	2.1	2.9	3.9	8.6

### SMR for those "seeking work" in different social classes (i) standardised for age only

Class	I	II	IIIIn	IIIIm	IV	V
SMR	79	109	113	123	155	150

### SMR for those "Seeking Work" in different social classes<sup>37</sup>

(ii) standardised for age and class

Class	I	II	IIIIn	IIIIm	IV	V
SMR	103	139	116	132	150	124

Social class standardisation reduced the excess mortality of all "unemployed" men in the sample from 36% to 21%. However, Jones then

<sup>37</sup> The difference between these two tables is that in the first one the "standard population" to whose death rates the rates in each class are compared is the population of **all men seeking work**; in the second table it is **all men**.

pointed out that when all the men seeking work whose usual occupation was "inadequately described" (those with inadequately described occupations had an unemployment rate of 50.6%) were removed from the analysis, the excess mortality adjusted for social class was 33% (i.e. SMR 133), a much smaller effect of standardisation. On standardisation for housing tenure, a variable which the LS team regarded as a good measure of social position, the excess was also reduced, to 27%. From this Jones concluded that "SOME of the mortality in the unemployed may be accounted for by their distribution by social class."

He next asked, was there a "reverse effect", which had been strongly proposed by Derek Cook in his paper, whereby those already in poor health were selected into unemployment. At this point Jones introduces, for the first time in a public report of LS results which I witnessed, the "reverse healthy worker effect", which was to play a decisive role in the rest of the debate:

Is there a health related selection effect? If that were the case, we'd expect the mortality rate to fall off with time, as the unhealthy subset died off. We'd expect the fall to be much more marked in the acute diseases, as well.

In fact, what we find is that for all-causes there is an INCREASE, not a decrease in the excess mortality associated with unemployment. Suicide does not decrease at all. Lung cancer does show a larger fall, but bronchitis and emphysema do not. If you standardise for social class, the all-causes rise in the differential mortality rate disappears. Social class gradients are wider in the second five-year period of observation than in the first. This is because the very sick HAVE died.

Here can be seen the emergence of the intricate relationship between the LS work on social class and that on unemployment, in relation to mortality. Jones concluded this section by observing that his figures could not be regarded as providing "strong corroboration of the view that the unemployed have a higher mortality rate because they are sicker in the first place". This cautious formulation was congruent with the general approach taken to this topic in the study.

Having dealt in this way with both the social class and the health-selection hypotheses, Jones added a new piece of information, on the mortality of wives of men seeking work in 1971. It had been found that these women also experienced higher mortality than that for wives of men in the whole sample,

an SMR of 120 for all-causes mortality, which included SMRs of 160 for accidents, violence and suicide, and of 157 for lung cancer. Now, whereas it might still be that somehow or other, men were selected into "unemployment" by ill health, it did not seem at all plausible that women would somehow be selected into marriage with "unemployed" men by similar criteria (we shall see that this was to be contested but it was not questioned at this meeting).

Jones concluded "That leaves us with explanation number (1.), the direct mechanism, if I can call it that." His overall conclusions were:

1. Mortality is raised in those seeking work on Census night 1971
2. Some, but not all, of the excess is attributable to social class, housing tenure, etc.
3. Some, but not all, of the remaining excess is explained by a health selection effect
4. Mortality is also raised in other members of the unemployed person's household
5. The residual excess is possibly attributable to unemployment per se.

How did members of the LS team account for their involvement in this work? Several of them expressed considerable detachment from whatever substantive issues they might happen to find themselves working on, including the present one: Far from declaring any particular "interest" in the question of unemployment and health, one told me:

this sort of work is just what I happen to be doing at the moment. This is very much just a job for me. (...) It's an odd feeling to have when you're working with people like the ones here who are really involved.

Despite what this researcher may have felt, another member of the team thought that it was very important I should understand his (as well as other peoples') work on unemployment and health as a contingent event in a "normal" scientific career. He gave an account of the organisation of research, and his own place within it, closely congruent with Latour and Woolgar's description of the "cycle of credibility".

Do you find that a lot of people are embarrassed by making a living out of other people's lack of a living? It's a bit of a bandwagon, isn't it? ... I was keen that [unemployment and mortality] shouldn't be pushed too far just because it is an OK topic and you can get money to do it. A lot of people have moved into it. A lot of people have been opportunistic in going into the field because it is an area that shows they are doing

policy-relevant work. Even if the results of the research are not going to lead to anything that's going to be implemented. ... This sort of thing happens in Medical Stats. Fashions come and go .. It [is] easier to publish because there [are] more people doing things like monographs and special editions of journals. These are the sort of practical reasons why people get into things .. you get asked to give talks and things like that because it is of interest to a wider group. So that you get a lot of positive reinforcement.

Despite (or perhaps because of) the clarity with which he saw the workings of the academic cycle of credibility, this researcher had moral and practical objections to merely being carried along by it. His account of his own involvement stresses the role of "chance". On the one hand, his arrival at City University was due to unexpected developments in his career. Additionally --

my first reaction to the idea of doing [unemployment and mortality] was that it was a waste of time. ... Our measure of unemployment is so weak. And mortality is an extreme measure of outcome, so we would be unable to see very much. What I didn't see at that time was that we could address the topic of social class, which was an issue then.

He himself had been attracted by medical statistics (as opposed to any other "applied" field) because of the opportunities it offered for "theoretical work". A disadvantage of working on the LS for him was that these opportunities had been reduced. An advantage, however, was

that you are talking to a very wide range of people, from theoreticians to complete pragmatists. You have to try and reconcile all their requirements .. It's a bit like being an entrepreneur.

Here he echoes the thoughts of both government statisticians and economists (inside and outside government) whom I interviewed on the importance of:

(a) being seen to have a firm grasp of the more abstract aspects of the appropriate discipline FIRST (you have to show you CAN do "theoretical statistics") before gaining access to policy-related work, despite what often seem to be a rather tenuous relevance of these skills in their more elaborate forms.

(b) an ability to "reconcile requirements" ("Seeing whether they want the Milk Tray or the Dairy Box") and to "sell one's wears" to policy-makers and others

As some of the civil servants whom I interviewed had warned, however, the

professional-statistician role described by some LS researchers did not extend to relinquishing the less convenient aspects of one's discipline in the face of administrative demands. For example, on one occasion while working for a health authority one told me he had:

proposed four measures [to be used in a resource allocation formula], like GHS self-reported sickness for example. It was quite fun. It was discussed by the Regional Team of Officers. Then they just chose the measure which met their prior intentions best. Now, if they had been Bayesians [laughing] ... Well, they were obviously not. They knew absolutely nothing about statistics. I wasn't so naive that I'd thought this wouldn't happen. It was the blatancy of it which surprised me.

He felt that part of the job of a member of his discipline was to make clear both the underlying "subjective and political element" in statistical work, and also to convey to non-statisticians the importance both of these elements and of the underlying technical assumptions with which they might not be familiar.

What the accounts of medical statisticians involved in the unemployment and health debate seems to show is a set of sources of dissidence from the ways in which they felt they were being asked to "operate". These are also visible in the wider profession, as can be seen repeatedly in presidential addresses to the Royal Statistical Society and other similar published documents (see, for example, Chandler 1984, Royal Statistical Society 1968, and, for a civil service "view", see Armstrong 1973). These are:

1. A desire to become more fully involved in the policy-making process (which led them into contact with pressure groups), but without having to abandon those aspects of their professional identity which often led to a critical and questioning stance,
2. A desire to be more involved in the substantive issues of the research in which they participated (whether these were the effects of work on health, health inequalities, drug trials or health service planning) rather than being "called in" merely to analyse data at a later stage in the research process (Cauliffe 1976),
3. A feeling that their discipline was being "misused" by people who either did not understand or did not clearly state the "underlying assumptions" involved, or the extent to which statistical "results" themselves involve an element of choice and decision-making.

The last point was sometimes made by statisticians whom I spoke to in the

form of "Bayesian jokes". Whenever Bayes is mentioned (which he quite often is see for example, Durbin 1987), the reference is to an alternative theory of statistics, which does not take it that everything in the world should be taken to be randomly distributed as a first assumption, but rather that the statistician explicitly BEGINS with certain ideas about how things "really are". These ideas are called "priors". The charm of the Bayesian approach will be evident from the obvious difference it would make to any group involved in a policy making process, as the "priors" will have to be derived from some knowledge or understanding of the substantive issues, which some statisticians wished to be credited with more often. The comments made above on health authority managers reflects a tendency amongst statisticians (and perhaps other scientists in fields closely related to policy making) to be reluctant to leave the "priors" entirely in the hands of politicians. Unfortunately, what is substantively gained is technically lost in this case, as the severe problems in characterising the distributions of test statistics and therefore of establishing the significance of results, have not been overcome by Bayesians.

This is not to say that in that group of medical statisticians which became involved in public health issues, as in "Manifesto" community medicine, there was no political source to the critical and innovative themes which emerged in the unemployment and health debate (amongst other places). There were certainly no members of the conservative party or workers at any of the right-wing "Institutes" involved in the development of the new ideas which emerged. But political "interests" only help us to understand the actions of participants when firmly combined with an understanding of the mediating roles of the subprofessions (in this case community medicine, health service management and planning), and the subdisciplines ("social" and medical statistics, medical sociology, health economics) in the relationships between policy-makers and wider social movements. Aronson (1982a) for example, has suggested that the type of moral fragmentation involved in the reduction of the problem of low industrial wages in early twentieth century United States to a cultural/psychological question of nutritional knowledge and skills was a product of "reformism". That is, the obligatory point of passage "working class nutritional ignorance" was produced by an alliance between the entrepreneurs of a new science and a reforming social movement which, however, wished to stop short at suggesting *structural* changes of a type which would have entailed redistribution of income. Following Aronson, therefore, we would



perhaps expect that there might have been "more research" under a social democratic or liberal conservative administration, but this would not necessarily mean greater "success" for an explanatory model which was only susceptible to "structural" interpretations.

At this stage in the unemployment and health debate, the effect of the formation of professional, disciplinary and personal alliances was to have created an intricate interpretation of the findings of the OPCS Longitudinal Study. The "wearing off of selection" was found extremely difficult to understand by many participants. It became an "obligatory point of passage" (as described in section 2.1), but only in the limited sense that it was not challenged during the rest of my observation of the debate. It was not so much accepted or taken-up by other groups, but rather constituted a barrier to further work, particularly to the work of "moral fragmentation" which depended on attributing a high level of mortality in a social group to the personal characteristics of group members. There WAS an agreement (tacit rather than explicit) that consideration of the wearing off of selection effects had to be included in future "serious" papers on unemployment and health, either to support it or attempt to refute it (though see Forbes and Macgregor 1987). Because few researchers felt able to do this, the "wearing-off argument" produced the beginnings of another impasse. Even sympathetic groups failed to take it up and use it in pursuit of THEIR objectives, and its factual "solidity" was accordingly fragile. However, a simpler construction of the message of SSRU Working Paper No. 18 was now taken up by the Unemployment and Health Study Group, and succeeded in once again raising the political profile of "unemployment and health".

### **7.3 "The Balloon Goes Up Again – but not so far this time**

By the next meeting of the Unemployment and Health Study Group, held on 22 August, the report "Unemployment Health and Social Policy", (the product of the seminar held in Leeds in November 1983), had been printed. A publication date was set for 17 September. Scott-Samuel had been sent a copy of the SSRU Working Paper on which David Jones' talk to the Institute of Statisticians had been based. This happened automatically, as his name was on a list (at SSRU) of "Interested Persons". The Study Group had been advised, by medical

sociologist/journalist Anne Karpf amongst others, to look for some "new research findings" with which to link the release of their Report, and it looked as if this would serve the purpose very well. After this meeting, Scott-Samuel, on other business in London, found time to speak to both the Sunday Mirror and the Guardian, in an attempt to associate the Report with the new research in the minds of journalists.

The press releases went out embargoed until 16 September, a Sunday, so as to "catch the Sundays" (attract the attention of the large Sunday newspapers). It described the Study Group as involving "doctors, policy-makers, academics and unemployed people". That is, it did NOT concentrate on the "mainly academic" identity of the group, which in other contexts tended to be emphasised. Specific policy measures advocated in the Report were:

- improved levels of benefits
- free public transport and recreation facilities for unemployed people
- job creation
- voluntary and flexible early retirement policies
- work sharing and shorter hours of work
- avoidance of psychological damage to unemployed people through stigma
- encouragement of support groups

In fact, the Sunday Mirror of 16 September carried nothing about unemployment and health. Later the editor wrote to Scott-Samuel apologising and explaining that "Princess Diana's labour problems" had usurped the space, but that they planned to do something at a later date. Nothing ever appeared in an edition of the Sunday Mirror, though later in September the Daily Mirror carried a small piece on the Report. On Monday 17th, the Guardian carried a large piece on page 2 which quoted Working Paper 18 along with a series of estimates made by Scott-Samuel of how many "unemployment-related deaths" had taken place in the year 1984 (Scott-Samuel 1984), and, last but not least, a report on the document "Unemployment, Health and Social Policy". In this case, at least, the strategy of information subsidy had been effective.

The LS team seemed rather pleased with what they regarded as a most enthusiastic response by the media. I asked explicitly what role had been played by the Study Group's strategy of hitching their report to the latest findings of the LS. Would they have made a decision to go public with the Working Paper at this stage? One LS researcher felt

It won't do us any harm. We had a discussion with Alex -- Alex

phoned us a couple of weeks ago. Then the Guardian came down and interviewed us last week... They were told what had been said at the Institute of Statisticians meeting .. They were given a couple of papers, and they've based the article on these, in addition to what Alex had done [on "unemployment-related deaths"] and the UHSG report.

In this researcher's opinion:

Basically the reason the Guardian liked it is that there is just enough of a hint .... that this is some sort of leak of some ""Official Report"

They admitted that talking to the press before publication in a journal had brought them in for "a bit of criticism" and that BBC TV had decided not to cover the story until the "Official report" was published, because it was government data. In fact, only one of the other national daily newspapers carried the story, despite the enthusiasm of the radio stations. This was The Scotsman.

Other reports of the Working Paper's findings were carried in the Morning Star ("Unemployment is Murder"), Labour Weekly ("Research confirms jobless die sooner"), and the Liverpool Echo ("Death on the Dole"). A BBC Radio London journalist for whom I did a brief interview (she was referred on to me by the UHSG) told me:

Once you've got down to death and suicide, what more can you say about it? People only want to hear just so much about things like this

which was a fairly common response of media-workers to the issue from this time onwards.

One civil servant's view of these events was that

I knew the balloon was going to go up again ... John Fox sent me a draft of his paper some time ago ... I don't think that in his sample he CAN be as definitive as a lot of people are interpreting it to be. All he can do is set out a strong case that there MAY be something. The first draft I saw was somewhat overstated.

Importantly, it seemed to him (as it did to me) that

There is much less hooaha about this issue this time round

However, this official's impression of the LS team's reaction to being the subject of an information subsidy was quite different to what they had told me:

from what I could gather, John Fox was by no means pleased about the way it came out in The Guardian. I suspect he will write a letter or something [in protest] ... Andrew Veitch always takes this sort of line, whenever anything about unemployment and health turns up...

and indeed it was commonly said amongst researchers that Veitch was not "typical" of medical journalists and would often give space to less orthodox views when others would ignore them. The civil servant quoted above seems to have been quite correct, also, in predicting that the media and parliamentary "balloons" would not reach anything like the heights achieved by use of Brenner and Fagin's work in 1981. The discrepancy between the two accounts of the LS team's reaction is puzzling. On 5 Oct, a member of the UHSG told me of a conversation that she had had with some people at Social and Community Planning Research, who have close links with City University. The SCPR people had said that OPCS reacted strongly to the "leak", by reprimanding members of the Working Paper's author group, and "reminding some of them of their civil service status". The situation did not seem to justify a great deal of official concern, in view of what was, in comparison to that received by Brenner and Fagin, relatively low media interest. However, at a much later date, some LS researchers looked back with a certain degree of dismay at the aftermath of events in September 1984, feeling that the SSRU had "had no peace" since then from official suspicion and scrutiny.

What may have motivated a certain amount of governmental disquiet over the possible effect of more bad news about the health of the unemployed was the revival of the more social-democratic or "wet" faction of the Conservative Party in late 1984. This revival had its major opportunity for expression at the annual party conference, which took place 8-12 October. But rumblings were apparent well before this date.

On September 25, a week after the "leak" of the LS findings, Hugo Young wrote a feature in The Guardian entitled "A valediction on the death of consensus in British politics". This article touches on many of the themes which were to overtake and incorporate the unemployment and health debate, and describes the movements in opinion which were ironically to make the debate both more relevant to general political discussion and less noteworthy in itself. Young laments several major changes in what he sees as British political traditions. Firstly, the Civil Service:

Civil servants now find themselves abandoning their customary role of testing the practicality of the politicians' objectives to destruction, and serving up instead the advice they know ministers want to hear

More generally, Young felt that:

the rules of engagement in public life have been rewritten. In particular, the concepts of neutrality and objectivity, so dear to the civil service mind and so close ... to the very essence of British civilisation, have been abolished.

He warns the Conservatives that if they so profoundly "politicise British society" as this, formerly quiescent public functionaries, such as Bishops, Judges and civil servants may be expected to start "fighting back". Echoing the sentiments of the government statisticians I talked to, a wider range of officials were beginning to formulate the idea of a duty to something beyond "the government", or as Young puts it, "the political imperatives of the moment" (Benjamin 1984, Boreham 1984, 1985, Orchard 1985, Hoinville and Smith 1985, Moser 1980).

Early in October, the Archbishop of Canterbury raised the temperature of "social issues" by giving an outspoken interview to the Times, in which he criticised the government's handling of the miners' strike as well as its general economic policies. According to the Times:

he denounced unprecedented levels of unemployment, despair and poverty in the community, inequitable sacrifices and those who "treat people as scum".

The Conservative Party Conference provided an opportunity for a resurgence of these more liberal ideas, and for the party leadership to be seen to be responding. Shortly before the Conference, Mr David Young, former chairperson of the Manpower Services Commission was appointed as "the Cabinet's own job creation expert" (according to The Times 8 October), to be a Minister without Portfolio and head of a Special Enterprise Unit "designed to promote job opportunities."

## 7.4 ESRC Workshop October 1984 – Compromises between experts

The miners' strike, revolt stirring amongst civil servants, protest from the Church of England and disquiet on the government's own left wing shaped the political context in which most of the leading academic protagonists of the debate on unemployment and physical health came together on 26 October, 1984. The occasion was another of the ESRC Employment and Unemployment Workshops at the Department of Employment. Two papers were scheduled, one by Fox on "Unemployment and mortality from the OPCS Longitudinal Study", the other by Platt on "Parasuicide and unemployment in Edinburgh 1968–1982". The organisers, as before, were Adrian Sinfield, Michael Hill of the University of Bristol's School of Advanced Urban Studies, and Chris Trinder of NIESR. Attenders included departmental advisers on economic and social issues from both DHSS and DE.

Fox began his presentation by giving some "historical background" to the LS work on unemployment, stressing that their early table showing the high mortality of the "unemployed" (i.e., men "seeking work" in the week before Census night 1971) had first appeared in a relatively obscure journal in 1979 (Fox 1979), and only been "picked up" in 1980 by the Cohort Study team. Thus, the controversial table's next appearance, in the Employment Gazette of September 1981 (in Ramsden and Smee 1981), had not been the LS team's doing at all. "The Department of Employment and the DHSS approached US," he related, "to see if we were interested in doing research on unemployment and health. However, in the end, we went to the MRC and other bodies for our funding (see section 5.2.2). He also pointed out that:

We did not return to this question of unemployment and health again until the beginning of this year -- and that was because I had been invited to a meeting in Australia ... Harvey Brenner was the invited speaker from the USA ... So I thought, I've got to find some new data, and I started looking at the unemployment data up to the 1981 Census.

Here Fox, aware that his audience included civil servants and professional advisers from the DHSS and Department of Employment, was clearly locating the "natural history" of the present work within the ACADEMIC cycle of credibility, and distancing it from the political debate. His introduction was

totally different to that given by David Jones in his presentation of a very similar paper, based on the same data, to the Institute of Statisticians conference. This was in accordance with both the very different audiences, and with the fact that circumstances were now more highly charged since the September 17th information subsidy.

In looking for his "new data" in the next five years' follow-up of the LS sample, it had been found that

the overall figure [for mortality amongst the unemployed] was surprisingly high

In characteristic "LS style", Fox went on to enumerate, first of all the "weaknesses of the study" (small numbers of deaths amongst the unemployed, weak and cross-sectional measure of unemployment, only 1971 data on unemployment available). This was followed by its "strengths": that it is not "ecological", it includes data on others in the index individual's household, it is prospective. The rest of his talk was broadly similar to that given by David Jones in July. At the end, he made an appeal to the audience for ideas for future work which they would like to see.

The discussion which followed will be reported in some detail because it represents a rare direct, public confrontation between the two "sides" of the debate, uninhibited by the conventions of academic publishing or a more formal conference setting. There seemed to be something of a "comprehension gap" between the LS team and its audience. (This may have been context-specific, but this could not, unfortunately, be tested, as at the more professionally homogenous Institute of Statisticians meeting so little time was left for discussion.) However, the most heated part of the discussion was on the question of "health selection", and here difficulties of comprehension and disagreements of substance were more or less indissoluble. One Department of Employment adviser was speaking for many participants when he described "selection" as "the crucial issue". He went on

Listening to the example you [Fox] gave, perhaps it is my fault, but I didn't understand it. ... much of the excess death is from smoking-related diseases -- and smokers are more likely to be unreliable workers as well as more likely to get lung cancer. How have you dealt with this sort of problem?

In his reply, Fox made the first publicly contested attempt which I witnessed to

explain the “unhealthy nonworker effect” and its “wearing off”, as applied to the problem of unemployment and health:

If you have a group of people with, shall we call it, a natural mortality level -- let's just assume for a moment that there is no social mobility. Then if, by your sample selection, you oversample people who are unhealthy at the start of follow-up, then mortality from this group should be high to begin with, but you'd expect it to go down [as the very sick members of the group “die off”, and the less sick recover]. On the other hand, if you take a group where you've selected OUT the sick people, you'd expect THEIR initial mortality to be LOW [in relation to that of the whole population which contains BOTH sick AND healthy], and then to rise back towards the natural [population] level.

Fox supplemented this explanation by some comments directly referring to his earlier work on the “healthy worker effect” (i.e., low mortality for all groups of “workers” at the beginning of cohort studies, which rises back to the “natural level” over time) as it had been used in studies of asbestos and other industrial hazards.

In the LS, if I take people by economic position and look at trends in health, there is [amongst the unemployed] a RISE in the level of mortality over time. If you take the sick group, it goes in quite the opposite direction. The “seeking work” group is artificially biased the other way from what you are suggesting [i.e. only the relatively healthy ARE seeking work as opposed to defining themselves as permanently sick] ... I'm not claiming this is conclusive. But the interpretation at this time is that our group is positively, not negatively, selected for health.

The same line of argument was then taken up by another government economist. What if there was some variable, not measured in the LS, which caused BOTH unemployment AND, say, suicidal tendencies?

the you will generate the kind of association you have found, even though it is not a causal relationship. We know that assortative mating is strong. Now, although I think the high mortality of the wives is one of the strongest aspects of your data, we cannot ignore the possibility that emotionally disturbed people, say, may marry each other.

Fox replied to this that there was no correlation found between the death rates, or ages of death, of spouses in the LS. The economist persisted:

We all know there is assortative mating by class, education, culture, etcetera. I don't know if that would be expected to lead



to similar levels of mortality.

But Fox was not to be moved. If these people had some reason for higher mortality, then eventually they would have to "die out", and thus their contribution to the group's mortality rate would "wear off" over time. In the long term, the mortality rate of any group some of whose members were "selected for poor health" would, therefore HAVE to fall.

If I've initially put in more of such people, some of them should die off in the first year, so next year there should be a smaller proportion of them in the whole group, and the next year even smaller, and so on ...

An economist asked

But what if the characteristic is one which isn't LIKE illness, but some trait which is likely to trigger mortality at more widely distributed times?

In response, Fox argued that there was no "medical model" of illness in use which could make sense of such a pattern of deaths widely and randomly spaced in time, which were nevertheless "triggered" by the same sort of underlying "factor" that could be used to explain unemployment.

Eventually, one of the economic advisers became more specific about what the "unmeasured factor" which could trigger mortality might be:

We only have fragmentary evidence on this, but what there is tends to suggest that people who are unemployed smoke and drink more heavily on average than the people in employment, but this is while they are EMPLOYED. When they are actually unemployed, they smoke and drink less. Now, this is precisely the kind of factor that will predispose people to unemployment and also kill them off earlier.

But Fox returned to the fact that the high death rate of the unemployed in the early period of follow up did not "wear off", which would be expected if a significant number of those found to be seeking work at one point in time were being killed off by something which predated their unemployment, no matter what this was.

Here I must make it clear that, in common with most members of the Unemployment and Health Study Group, as well as a wide range of other participants in the debate, I did not fully understand this argument in late 1984.

The phrases in square brackets within the quotations from my field notes of Fox's talk have been added at the time of writing in order to make comprehension easier for the reader. Understanding the "wearing off" argument was something which happened to me (and others) almost in the manner of a "conversion experience" or paradigm-switch (in my case, during the autumn of 1986). What now happened in this Workshop also seemed to me at the time to resemble a conversion experience on the part of one of the economists. But it did not take place on the basis of the arguments derived from occupational epidemiology. Rather, it was a product of the perceived strength of the argument that spouses of the unemployed were (despite "assortative mating") NOT very likely to be similarly "selected" for poor health. It was also based on the perception that the work embodied in the second paper (Steve Platt's) was amenable to a different kind of research programme which might appropriately "domesticate" the academic debate.

After the usual long tea-break, during which the participants tended to break up into "camps", statisticians on one side and economists and government advisers on the other, Steve Platt spoke on the findings which he and Norman Kreitman had just had published in the BMJ, four days previously (Platt and Kreitman 1984, see also Platt and Kreitman 1985, Platt 1986). As was stated in their second draft, submitted in August, Platt now repeated that these were "preliminary results" and that a fuller version would be published in Psychological Medicine in the following year. He summed up his talk:

I must make it quite clear that we tend to conclude -- how can I put this now? -- that our findings are not incompatible with a causal role for unemployment. That is the most conservative way of saying it that I can think of. But we point out that we are not putting forward a monocausal theory. We know that lots of these people have alcohol problems and criminal records and that kind of thing. We are aware that there are self-selection factors involved.

After the talk, the second person to ask a question was a prominent member of the "other side" of the argument, one of the economists. He felt that

The question is, what ought one to be doing about or in response to this balance of evidence? That's a very big agenda. I think the SAPU people find that, for young people, going in a YOP course has as good an effect as being in employment

The economist continued to enumerate a series of possible "alleviations": extension of the long-term rate of benefit to the unemployed, Community Programmes, wider availability of part-time work. If the "poverty effect" visible in ecological data were significant, then the first measure might be the most important, he suggested.

After some more questions and discussion on the combined social effects of recession and inflation on people other than those actually unemployed, Platt stepped out of the specialist social-psychologist role, as if to extend an appeal for dialogue to the economists present.

I'd like to say something about inflation -- here I'm going to get sat on by an economist -- We use unemployment as an indicator of the state of the economy which is reasonably reliable. What worries me is the extent to which it gives one a true picture of the state of the economy at the present time. Whichever indicator you read: inflation, GNP, income per caput, etc., you get a different impression ... Is this very silly?

To which one of the economists replied:

It is very sensible. (...) You've hit on something very important. There are clearly times when unemployment is high and real incomes are growing (...) Unemployment rates are good indicators of the state of the unemployed, but not for the state of the employed.

This exchange illustrates the beginnings of a promising "translation" -- a member of each group tentatively extending an offer of their respective definitions of the situation as a "resource" to the other side (see section 1.5).

The rest of the discussion centered around the psychology of parasuicide and reflected both the interests of a large section of the audience, and of the MRC Unit in which Platt worked. When the meeting ended, some participants "hung around", forming small groups. I asked the economist who had proposed "policy changes" (I shall call him "E") if I had heard him aright? Was he really proposing a change in the agenda, from academic argument to discussion of concrete measures to protect that health of the unemployed? He replied, firmly, that he was. The sort of research now needing to be done was exemplified by Jennie Popay's study of how health visitors and social workers dealt with the problem of having a growing proportion of unemployed in their caseloads. More research was also needed, however, on "precipitating/mediating effects and how to intervene". I asked if he thought there would now be a lobby within the DHSS for the extension of the long term rate of benefit to the unemployed.

But this, he felt, was crying for the moon, "It's a question of priorities". At this point we were joined by John Fox, who was clutching a note passed over to him by E during the discussion of Platt's paper. "Can we [the LS team] rely on you to say this sort of thing in the Department?" Fox asked. The economist replied, cautiously, "I'm convinced. It's just a question of whether I can be SEEN to be convinced." Fox urged him, "It's the sort of thing we need you to say". (The LS researchers were at this time negotiating with the OPCS for permission to carry on linking post-1981 vital event data to the 1971 sample). But the economist replied, "Come on, John, you and I know ... that the DHSS is very hard to move on this sort of thing."

In this exchange, which exemplifies a "failed" attempt at "translation", there is a striking example of how research and policy objectives can become confused. Fox was asking E to "lobby" within the DHSS for the respectability and usefulness of his STUDY, in order to obtain further co-operation from OPCS. The claim "unemployment damages health" was not being put forward by Fox as a "moral claim" (for better benefits, or more training schemes etc) here, but as a "knowledge claim", and as a bid for resources to pursue the study<sup>38</sup>. However, E immediately interpreted it (quite correctly, from HIS point of view) as something that would involve a government department in possible expenditure commitments which "any civil servant knows" is a difficult thing to do. So that E's conversion, wholehearted on the intellectual level, could be "seen to be" only partial, although the sociologists present at the meeting were astonished that someone in his position had gone even that far.

Both E and Fox agreed that research

does not really influence the Treasury. If they decide to change policy -- say to extend the long-term rate to the unemployed, they'll do it for some other reason. Then, of course, they'll quote research on health effects [of unemployment] as a justification. But they'll never do it FOR that reason.

Now, this seems to contradict some of my previous conversations with advisers to "spending departments", who said they had first taken an interest in Brenner's work because it might come in useful in their battles with the

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<sup>38</sup> Although these objectives are not mutually exclusive.

Treasury. What seems to be being said here is that officials in government departments do not become either interested in or convinced by research merely because of its "quality". As Knorr-Cetina has pointed out (see section 1.5), acceptance by peers is not a guarantee that a piece of work will advance a scientist's cycle of credibility, in the sense of facilitating claims to resources. Rather, attitudes towards research depend on the ways in which projects and their (actual or potential) findings fit into the wider strategies of, for example, funding bodies, or government departments, or of groups within these organisations.

The status of research "findings" as "resources" must be negotiated, it is not self-evident and cannot be predicted from the reception of research by the academic community alone. Policy advisers in government departments select research findings STRATEGICALLY, rather as the writers of academic papers select the works to which they will refer (Law and Williams 1982). No longer was E sceptical of "the facts", but only of their "usefulness" to those involved in formulating and defending policies, and those wishing to ensure their own "organisational survival" as policy advisers, given the changed political climate since Brenner's first appearances in Britain. But, as Knorr-Cetina, once again, has pointed out, these decisions are also a part of what constitutes the "accepted state of knowledge" at a given time. For reasons he admitted were "tactical", E hesitated to volunteer to be "seen to be convinced". And at no future time did he write for publication any retraction of his previous widely quoted papers which had thrown such severe doubt on the idea that unemployment could "directly" affect health. So that his strategic orientation towards the knowledge-claims put forward by the LS researchers also had consequences for the debate on "the facts".

He went on

It would be mistaken to aim this stuff [the findings of the LS and of Platt and Kreitman's study] at the macro-economic debate ... If this government wouldn't make a U-turn in 1981, how much less can they afford to now?

Rather, he felt that if research was to continue being seen as a useful adjunct in the debate on social policy relating to the unemployed, it must be removed from the discussion on ECONOMIC policy, and redirected towards problems of "intervention" and "alleviation". Useful research should therefore in future include:

- 1) improved estimates of the costs of labour market policies, including costs at the "macro" level,
- 2) recommendations on how to carry out limited interventions directed at vulnerable groups

In other words, to be "useful" at this stage, research must partake in the process of moral fragmentation described by Manning (1985). Potentially, Platt's work was more amenable to this kind of change in emphasis than that of Fox and colleagues. Although I did not hear their conversation, I noted at the time that E spent far longer talking to Platt than to anyone else, and it was Platt's paper, not Fox's which precipitated the "I'm convinced" note.

"Moral fragmentation" was also in general accord with a strong strand in the research programme of the MRC Unit for Epidemiological Studies in Psychiatry, as shown by their Progress Reports to the MRC. For example, their work on depression amongst women showed that environmental factors and threatening life events produced depression only in those "already predisposed" (see Ingham et. al. 1986, 1987) Whatever they may have FELT about it, the LS team were constrained by their data to continue to study "structural factors"; that was all they had. The data available in the Decennial Supplements carried the mark of a much earlier social-problem process (see Szreter 1986) concerning differential FERTILITY<sup>39</sup>. As such, it simply did not contain items which could be adapted for work of "moral fragmentation". All that was open to the LS team at this point was an increasingly "technically fragmented" defence of their position, which became more and more difficult for EITHER side of the debate to integrate into their value claims.

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<sup>39</sup> I was told that the Chief Medical Statistician, Dr Michael Alderson was lobbying for the inclusion of smoking, drinking and exercise measures in future Censuses. This would have provided material of a more suitable kind for what some officials saw as the appropriate level of policy debate.

**CHAPTER 8**  
**STAGE FOUR – THE SOCIAL**  
**PROBLEM CLAIMS ARE ACCEPTED (OR ARE THEY?)**

## **8.1 Stage Four (A): Acceptance**

It would seem that those who had for long suspected, and sought to demonstrate, that unemployment damaged physical health, and wished to persuade policymakers of this were now taking the offensive. However, what we shall see in this chapter is the debate entering the fourth phase of Manning and Downs' model, which has been characterised by Downs as a "twilight zone" of declining interest. Added to this was the tendency, which became strongest at this point, for the debate on unemployment to regress towards the older one on health inequalities. This did not happen because of any scientific work "successfully" questioning the knowledge claims of the BRHS, the LS or the Edinburgh MRC Unit, but rather as part of the increasing tendency of this work to be interpreted differently according to the uses made of it by different groups, and not to be directly challenged.

### **8.1.1 Research to policy: in the balance**

On 14 November 1984, the Unemployment and Health Study Group meeting noted with satisfaction that Kenneth Clarke, both in a letter to the Nuffield Centre for Health Services Studies who had published the UHSG's "Leeds Report", and in his speech to the MIND annual conference, had admitted that unemployment was damaging to health. But, the minutes note, "the change in government attitude had not resulted in any policy change".

On 8 December, "Unemployment and mortality in the OPCS Longitudinal Study" by KA Moser, AJ Fox, and DR Jones was published in *The Lancet*. But by this time, there was not a great deal more to be said about the 1971–1981 follow-up of the LS's "unemployed" men and their wives. The material in the published paper was essentially the same as in the working paper. One difference was that Peter Goldblatt's name had disappeared from the list of authors, removed on advice from his colleagues at the OPCS, where he (unlike the other authors) was employed as a full-time civil servant. The "Introductory"

section had also been shortened and considerably changed. Most important, perhaps, was the limitation of the discussion on "inadequately described occupations" to a mere reference to the working paper. In the discussion of "selection", the published paper is rather more explicit than the working paper or David Jones' rendering of it. The "wearing off" to be expected in a group selected for poor health is, in the Lancet version, spelt out as due to

the high initial mortality of those selected on the basis of ill-health [which would mean that] the proportion of sick men in this category would decline over time ... as has been observed in other areas of our work.

Both in the Lancet paper and the working paper, a far more cautious attitude is taken towards extrapolation of the findings to the unemployed of the 1980s, than that exhibited in the "leaked" Guardian report of September 1984. As unemployment has become a "more common experience", has its severity decreased, due to a reduction of stigma? On the other hand, average duration of unemployment has INCREASED. The Lancet paper promises that further analysis of the LS cohort will answer this question in due time (which is, of course, by way of being a claim for resources).

There was little media response. The UHSG produced a deliberately co-ordinated onslaught on the letters page of The Lancet, with letters coming from Steve Watkins, Silvia Tilford (a member of the Leeds Report drafting group), Gill Westcott, and Scott-Samuel. The latter included in some detail (including two tables) the extrapolation of Moser et. al.'s findings to the 1980s unemployed population. Even taking the lowest and most conservative estimate, Scott-Samuel claimed, there had been an annual excess of deaths "due to unemployment" of 1034 (combining unemployed men and their wives). "For every 2000 men seeking work, 1.94 men and 0.98 wives will die each year as a result of unemployment" he concluded, offering a Brenneresque ready-reckoning method for any local planning department, or indeed, pressure group, to use.

The Labour Party did take up the question, independently of the UHSG. Rather dramatically, Labour Front Bench health spokesperson Frank Dobson is quoted (in Labour Weekly, 4 January) as saying

At first the government pretended that there was no link between unemployment and ill-health and deaths. Recently they



have had to begin owning up to those links ... That means that now, through their policies, they are knowingly killing people.

On 10 January, New Scientist devoted a full page to a feature entitled "Death on the Dole", reporting that the LS results

will be taken up by Opposition MPs in the coming weeks as further evidence of the inhumanity of the government's economic policies

As one might expect, the "wearing off" argument makes its first "popular" media appearance here, in a popular-scientific journal. Unfortunately, it is not phrased in a way which seems well calculated to clarify the idea, even to the scientifically trained uninitiated.

At the Unemployment and Health Study Group's first meeting of 1985, on 16 January, it was felt that:

...we were not 'over the top' yet as regards government attitudes, research findings, and the Unemployment Health and Social Policy report [the "Leeds Report"]. The acknowledgement by Kenneth Clarke of the health-damaging effects of unemployment needed to be translated into action; the steadily more conclusive findings of research needed wider dissemination.

Against this background of debate, the last face-to-face confrontation between members of the "core group" took place on 1 February 1985.

### **8.1.2 A Needle and A Haystack**

This workshop comprised a small invited audience at the Centre for Economic Policy Research in London on 1 February 1985. An eminent group of speakers had been invited from various parts of Europe, their expenses paid by the DHSS, as part of its ongoing support for the Centre's programme of research on "Human Resources". The organiser of the "workshop" was Prof Roderick Floud of Birkbeck College, an economic historian, who also chaired. It took place in a cheerful and lively atmosphere. By now, most of the researchers in the "core group" knew both each other and the representatives of "Manifesto" Community Medicine who had been most involved, Watkins and Scott-Samuel. It was a welcome opportunity for widely dispersed members of the network to meet. A considerable number of notes were passed back and

forth during the more formal sessions, and breaks were busily occupied, both by friendly chat and by what was described as "horse-trading" between academics and civil servants (three were present from DHSS and one from the Department of Employment).

The first paper was Derek Cook's, another version of the Institute of Statisticians' paper. Once again, he made heavy use of the dietary study of Doyle and Crawford, somewhat strangely in view of the fact that this study does not specify whether ANY of the breadwinners of the Hackney or Hampstead samples were actually unemployed. On this occasion, he also criticised the suggestion in Gravelle, Hutchinson and Stern's 1981 paper that a "robust effect" could be isolated in time-series work by "better statistical techniques". Cook felt this was a "pious hope", and that "superior statistical technique will not be the answer".

Cook's discussant was Jon Stern, who felt it had been a "very good exposition of the problems as I see them". He agreed that more time-series analysis was pointless, despite what had been said in the paper of which he was co-author ("I didn't write THAT sentence") and felt that

The fundamental methodological problem is what is known as selection bias.

By placing the problem of "selection" back at the centre of debate in this context, Stern was adopting the strategy consistently used by the economists, that of "moral fragmentation". In this context, the question "Does unemployment cause ill health" is fragmented into questions concerning "Who are the unemployed and are they at risk of ill health for other reasons?" (the "ceteris paribus problem"). This strategy had been clearly visible in the discussion at the October ESRC workshop. It was also extensively deployed in another related debate on the "Black report" and the causes of social class differences in health<sup>40</sup>. It was not just a question of whether unemployed people were physiologically "diseased". The meaning of "selection" shifts according to the argument in which it is used, and the people who use it, but here it meant both "diseased" and also "unfit" in the eugenic sense (which

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<sup>40</sup> see discussion in section 6.2.1, also Stern (1983), Himsworth (1984).

comes close to the economists' concept of "low in human capital"). Stern now felt, however, that

we do now have three large scale good studies. It would be stupid to deny that there is some effect of unemployment on physical health and mortality. The effect is probably small rather than large ... But we have found the needle in the haystack.

Policy advisers had, it seems, adopted the studies which have been mostly extensively discussed in Chapter 5 of the present study as the basis for what now seemed to be an emerging consensus. Stern went on to ask where "we should go from here". He was in favour of "small scale studies" and "action research" to investigate "mediating effects", that is, the pathways by which the now admitted "ceteris paribus effect" of unemployment on health operated.

One of the officials present then took up a theme of the discussion following the October ESRC workshop:

What do we mean by "policy"? As a DHSS official, policy in this area means affecting the kind of policy variables that are under the control of the DHSS. One problem is that the health effects have been used as a weapon in the war about macro-economic policy. Fine, terrific. But THOSE kinds of policies are under the control of the Prime Minister and the Chancellor of the Exchequer ...

Further interesting information and discussion was forthcoming from papers that followed, by Charles Webster, Irvine Loudon, Urban Janlert, Jurgen John, and Noel Whiteside<sup>41</sup> But from the point of view of the present account, the other important discussion took place between Platt and Gravelle, the "discussant" to his paper, chosen by Floud.

Platt's paper was scheduled as the first after tea. During the break, the "discussant" to the paper, Hugh Gravelle foreswore the social mixing, took his tea back into the seminar room, and proceeded to write a series of equations on the blackboard, which he left there. The paper once again presented the Edinburgh Regional Poisoning Treatment Centre study, with both the "ecological" and the time-series "individual" data (see Platt and Kreitman 1985). Offering a "translation" of the work as a resource to the range of workshop

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<sup>41</sup> Much of which has been written up elsewhere: see CEPR Bulletin March 1985, Whiteside (1987).

participants, Platt began by remarking

The origin of the project was in the growing interest in unemployment and health and the thought that I could exploit a unique data set

The main points of the paper that are relevant for the discussion which then took place were as follows:

1. Unemployment and parasuicide were highly correlated over time, at least until 1983,
2. As unemployment rose, the proportion of parasuicide cases who were unemployed also rose
3. Over time, the relative risk of parasuicide amongst the unemployed (compared to that among the employed) began at a very high level, then fell, and then stabilized at around 11 to 1
4. Cross-sectional data showed that areas of high unemployment tended to be areas with high rates of parasuicide, but this correlation was reduced to statistical insignificance when a measure of "poverty" was introduced as a control variable.

In reply, Gravelle re-emphasised the economists' distinctive approach:

... you have to start with basic models. Firstly a production function -- an aetiological model that summarizes your hypotheses about the relationship between unemployment rates and parasuicide rates ... Secondly, you need a model of behaviour.

He went on to give the fullest and most explicit account to date of "the economists' approach" to the problem of the relationship between unemployment and health, which most clearly demonstrates the technical accomplishment of moral fragmentation.

Gravelle's first two criticisms of Steve Platt's paper were purely technical. He pointed out that the proportion of parasuicides who are unemployed is bound to increase as the unemployment rate rises (just as the proportion of, say, red-headed people who are unemployed would increase as the rate of unemployment rises). Similarly, the amount of parasuicide in the population as a whole which can be ATTRIBUTED to unemployment (the "attributable risk") must rise as unemployment rises. Next, as in his critique of Brenner, Gravelle proposed that if Platt and Kreitman's time-series was broken into two, the correlations would disappear for each separate period. Turning to the "ecological" data, Gravelle picked up the point that when "poverty" was controlled, the cross-sectional correlation between unemployment rates and

parasuicide rates in Edinburgh enumeration districts disappeared. That suggested to him

severe problems of multicollinearity. That means that all the results which do not include poverty as a measure must be biased.

There was an anomaly, he thought, between the time-series and the cross-sectional data. This was that across areas with different rates of unemployment, parasuicide rates rose steadily: the higher the unemployment rates, the higher the parasuicide rate. But across TIMES with different rates of unemployment, the RELATIVE risk of parasuicide behaved less consistently. As the unemployment rate rose, the RELATIVE risk of parasuicide first fell, and then stabilised. He proposed that this anomaly in fact offered the key to explaining the "true" link between unemployment and parasuicide. Namely, at the beginning of the period of the Edinburgh study, the high rates of parasuicide amongst the unemployed were entirely due to "personal characteristics" (as Platt himself had argued in 1982). The finding that parasuicide was highest amongst HIGHER social class individuals who were unemployed could also be explained in this way -- they were the ones with the most "personal problems" (i.e. they were unemployed despite a relatively favourable labour market position). People with "personal problems" were also concentrated in poor areas. As unemployment rose, the second of the two possible determinants of the "propensity to unemployment" (i.e. the demand for labour) took over from the first ("personal characteristics"), hence one would expect the "new" unemployed to have better mental health and a lower rate of parasuicide. This alternative explanation had always been latent in Platt and Kreitman's analysis, as were other kinds of moral fragmentation of other kinds of social problems (depression, alcoholism) in the work of the MRC unit.

This was an impressive presentation, the effect of which was heightened by the presence of complicated-looking equations on the blackboard. The intensity of the confrontation between Platt and Gravelle meant that the closing discussion of "policy", introduced by Jennie Popay, was conducted in a somewhat muted atmosphere. Popay felt that

You're asking for more research in this area than ever gets asked for to justify policy change in other areas. The only similar case I can think of is with inequalities in health. You ask for so much rigour, that you end up with rigor mortis.

Floud then turned to “our colleagues from the DHSS”, and asked their opinions. One research administrator felt, like Stern, that

The approaches we can adopt have to relate to services. Some measures to help improve the level of living of people who are unemployed is the direction we’d prefer.

In contrast to the research manager, an administrator thought

There are some policy implications we can take on board and some we cannot. The implications of raising benefit, if that was what the research implied, are such that the present government would be unlikely to envisage, let’s face it.

Gravelle himself was more cautious about “intervention”,

Unless you have got some model, just going in and intervening might not tell you much.

he warned.

Although the technicalities in the economists’ account were just as elusive and opaque to a non-specialist audience as those of the statisticians (the “wearing off of selection effects”), there was a crucial difference. The economists’ symbols and equations were relatively fluently translated into a moral language which the lay person (and policy-makers) could understand (basically, the unemployed have a set of “personal characteristics” of an ambiguously moral/natural kind which unfits them for normal life in society and accounts for both physical and economic vulnerability). This translatability, in Latour’s terms, rather than JUST the way in which their model individualizes social problems (as discussed by Manning, and which I have termed “moral fragmentation”), seems at least partly to account for the success of the economists as policy advisers. Their approach was not LESS technical than the approach of the statisticians, as Gravelle had demonstrated. But it attracted allies both because of what it could be translated into, and the active way in which this was done by the economists (“selling their wares”).

The statisticians’ strategy had different effects. It concentrated on repelling assailants by means of its technical complexity and its authors’ intimate understanding of the large and complex data set involved. There was, of course, a potential moral translation of the statisticians’ analysis. But this translation would have a tendency to attract allies of a more problematic kind (as discussed in section 3.5 and 4.3.3. above). Inherent in the very nature of the data available to the LS team was a tendency to highlight “structural”

explanations of health inequalities. The data set simply did not include information on such lifestyle or behavioral factors as smoking, drinking and exercise. The resulting "structural" analysis had **at times** attracted such powerful allies as the Royal College of Physicians, particularly its Faculty of Community Medicine. When this happened, the "structural" approach was useful to the scientists, in their pursuit of tactical, "trans-scientific" objectives. But there was a fine line which divided the usefulness of the LS to the social problem claims-making activities of powerful and prestigious groups in medicine, from a more dangerous exploitability of the results by "ginger" factions *within* the medical subspecialties and other professionalising groups such as health education. Insofar as these latter "customers" tended to emphasise a more overtly "political" interpretation of the LS results, the scientists pursued relationships with *these* allies in a more hesitant fashion. They might, after all, subject the scientists' findings to just as much "distortion" as any other group (for example, health service managers or officials). The different modes of "fragmentation" employed by the different disciplines can therefore be understood as partly the outcome of the ways in which these two subdisciplines related to different "customers" or clients.

The evening after the meeting, considerable disquiet was expressed by some participants at the outcome of the day's proceedings. As one put it:

I don't think what Hugh did was on. Steve has always played it straight, always played the game according to the rules. If Hugh was going to attack him like that, at least he should have warned him.

Plans were made to liaise with Platt over "how to get round" the "problem" pointed out by Gravelle. One statistician's opinion was that Gravelle's presentation had been "rubbish", implying that it was not really worth bothering about. Between this day and the end of my period of observation of the debate, Gravelle's rebuttal of Platt and Kreitman's work was never published in a refereed journal, and Platt was therefore under no obligation (according to "the rules") to reply to it. He sent a copy of the equations to Cook, who found the notation confusing, and both researchers were too busy to devote the necessary time to clarification. The links proposed that evening between Platt, Stern and Cook were never re-activated.

### 8.1.3 Stalemate

An account of the CEPR workshop was written up and published in the next edition of the "CEPR Bulletin" in March. It came into the hands of the Unemployment and Health Study Group in a roundabout way. A copy WAS sent to Michael Meacher in his capacity as Shadow Secretary of State for Health and Social Security, and his research assistant forwarded it to Scott-Samuel, whom she knew as a member of the Front Bench advisory group with this special interest.

The comment on Platt's paper was that

his methodology was heavily criticised in the discussion which followed [his paper]. Without further research, these conclusions will not gain widespread acceptance.

and the report of the workshop concluded

It is necessary to ascertain what aspects of health are affected by unemployment and what remedial measures can be taken to protect the health of unemployed people.

The outcome, at least in the CEPR Bulletin's account, was a striking reversal of that of the ESRC workshop of October 1984. One of the major pieces of research in the debate was now redefined as something which "would not gain widespread acceptance". However, contrary to this conclusion, Steve Platt continued to be invited to address important academic and professional meetings (such as the Royal College of Psychiatrists' annual conference in July 1985) throughout the rest of my period of observing the debate, and he was also invited to write an editorial on the topic for the British Journal of Psychiatry, which appeared in the late summer of 1986 (Platt 1986b). The notion that unemployment increased the risk of suicidal behaviour continued to be widely accepted in subsequent literature as one of the "firmest" findings of the entire corpus of research on unemployment and health.

It seemed as though the academic debate had now reached a point where the two "sides" were relatively polarized, in terms, at least, of the client or "customer" groups which formed salient parts of their "trans-scientific fields". To some extent, this gave the impression that the two "sides" were also taking on the aspect of opposed social groupings, and it is true that some social



contact between members of each "camp" though not a very great deal, did persist beyond the more intense phase of the debate. Those who believed that the balance of evidence pointed to a "direct effect" of unemployment on health were sociologists, medical statisticians, and community physicians oriented towards "social medicine". They made use of an "incremental" criterion of truth, looking at the accumulated "balance" of the studies that existed. The ultimate technical justification of this position resided in the notion of an "wearing-off of an unhealthy nonworker effect" which should be present if "selection bias" is the "real" explanation of patterns of mortality differences between groups. This argument, however, was not really understood by most of those in the wider issue community who favoured a "direct effect", at least, certainly not by the spring of 1985, and no-one outside Fox's Social Statistics Research Unit ever used this argument in their own future work.

The argument against the reality of a "direct" effect of unemployment on health was put forward by economists, who worked by techniques of "modelling", using an explicit theory about "selection by personal characteristics". The strongest evidence AGAINST the selection hypothesis was that the death rate from accidents, violent deaths and suicide, the major causes of excess deaths amongst the younger "unemployed" in the LS sample (who had higher relative, though lower absolute mortality rates than older men) showed no sign of "wearing off" with time. Men "unemployed" in 1971 had just as great an excess of mortality in 1981 as in 1976, when these causes were considered. The argument was that if a proportion of these men had been selected into unemployment by some "propensity" towards violent death, then those WITH the propensity should have been "killed off" by it as time went on, causing the death rate for the "unemployed" as a whole to fall back towards the average in the later stages of follow-up. If the higher death rate of the "unemployed", that is, could be accounted for by the deaths of those men with such a "propensity", then the deaths of these men should leave a group purged of their influence. This could not be tested in Platt and Kreitman's data set. Nor was it ever tackled head-on by the economists. In theory, data from the Regional Heart Study might have thrown some light on the question, but events during 1984 had meant that the priorities of the BRHS and of Cook in particular, had changed.

In the autumn of 1984, the Regional Heart Study application for a further five-year programme grant from the MRC (its third) was unsuccessful. As a

consequence, Derek Cook had to abandon his work for a Ph D using the follow-up data on unemployment and mortality experience of the 7735 men. His new topic was to be an investigation of lung function in relation to social class, town of residence, and smoking. Cook's decision to re-direct his efforts was taken quite abruptly. The Lancet of December 8 1984 had published an optimistic letter in which he spelt out the promise of the study in relation to the question of the relationship between unemployment and health:

The suggestion arising from this study is that those men who have early evidence of disease may become unemployed more readily and remain unemployed for longer than men who are healthy. This possibility cannot be confirmed from the preliminary cross-sectional analysis, a more dynamic view of the unemployment/health relationship is needed.

To obtain this "dynamic view", the BRHS researchers were using information from a postal questionnaire to all men at five years after entry into the study. The questionnaire had received a very high response rate (98%), and, in conjunction with the measurements made at initial examination, it was hoped, would enable them to

examine the extent to which men have been selectively forced out or kept out of the workforce because of ill-health ... The Regional Heart Study should be able to provide detailed information on the dynamics of the unemployment and health interrelationship and to quantify this 'selection of the fittest'.

However, the loss of its MRC grant now made it uncertain whether there would be resources even to finish sending out and analysing the follow-up questionnaires which were to have provided the data for Cook's Ph D.

In the event, the Regional Heart Study did receive generous funding from other bodies, such as the British Heart Foundation. However, the need to change the emphasis of the work and to be more willing to prepare papers for publicly visible occasions produced a new set of priorities for Cook. He firmly believed, by late 1986, that smoking amongst the unemployed could be shown to account for both a raised risk of unemployment and for the "randomly triggered" higher mortality, evenly spaced out over time, found amongst those men in the LS who had been unemployed at the time of the 1971 Census. The spacing of the "excess deaths", he felt, could be explained by the fact that people had taken up smoking at different ages, and so reached the end of the "latency period" for lung cancer (the cause of death most in excess of average

amongst the middle aged and older “unemployed” men in the LS) at different times. As the BRHS only dealt with older men, Cook made no claims about ways in which smoking could account for accidental and violent deaths, which were the most in excess amongst younger “unemployed” LS sample members.

For the time being, then, the sociologists and statisticians had reached a position of stalemate in relation to the economists. There was no pressing reason for effort to be put into breaking this, as for one thing, the academics amongst them inhabited rather different social worlds -- they attended different conferences (on the whole), published in different journals, and, on up to now, had been applying for funding to different organisations. Potentially, Cook’s theory could have formed a bridge across which medical statisticians and economists could at least have argued with each other, over the importance of “personal characteristics”. In substance, Cook agreed with Gravelle, yet he spoke the methodological language of the LS team and the Edinburgh MRC unit. The important difference seems to be that, as a medical statistician, Cook’s “natural” client group was community medicine, and he was therefore in a sense “tied” to one side of the debate, not by any cognitive factor, but by virtue of where his discipline stood in relation to the professional boundary-dispute<sup>42</sup>.

#### **8.1.4 “Unemployment and health in general practice”**

One new study did appear in the debate in 1985. This was the “Calne study”, a longitudinal case-control study in which the cases were all patients of a single general practice. In 1982, Harris Meats, the major employer in the country town of Calne, Wilts, made their entire workforce redundant. The first reported findings of the study were that redundant workers and their spouses were significantly more likely to consult the GP, but not AFTER being dismissed. The rise in consultations took place during the extended “anticipation” period during which workers were aware that the factory was likely to be closed. Redundant patients were, furthermore, more likely than

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<sup>42</sup> The one medical statistician who eventually published a paper showing “no effect” of unemployment rates on mortality rates in different geographical areas (Charlton 1987) was, at the time of publication, working for the DHSS. There is no intention here to suggest that the debate *inevitably* took place between disciplines rather than within them, rather to describe the ways in which debate was socially organised.

controls to be seen as sufficiently ill to warrant referral to hospital out-patients departments for specialist advice.

In March of 1986, a fascinating account written by Beale about the process by which this research came to be done, was published in the BMJ (Beale 1986). It is a classic "discovery account", in that the "idea" is depicted as "abruptly" occurring to him in the spring of 1983 while he was "giving the lawn its first haircut".

Like several other researchers (such as Fox) in their "public" discovery accounts, Beale distanced himself from any possible "political" motives. However, he was aware that the imminent closure of the major employer in his area might "be affecting our practice workload." This accomplishment of motive is followed by a gesture towards "science":

In Cambridge I had been allowed the ultimate privilege of a British science education. The master of my college had won a Nobel prize for work on DNA ... I remembered with pleasure the research project I had done for a year after Second MB; I had learned to ... define aims, to shuffle index cards, to tabulate results ... I was familiar with the infrastructure of science at least

His account of his own "Pilgrims progress" through the project is disarmingly honest about mistakes, confusion and "cheating". Yet it does portray the process as cumulative, as a progress through error towards the discovery of truth "out there".

Beale described his mood in various emotional terms:

I was hooked ... Creativity is close to madness it is said. I could not have survived the next year without a demonic mania that had developed. I now had an obsession.

After the tedious work of looking through company files and medical records to identify his cases and controls, and trace their medical histories:

I began to be naughty. I craved for results and repeatedly compiled data from incomplete samples.

To his horror, even the completed work showed no change in illness following redundancy: "months of work and nothing to show for it". However, the fact that the practice's own cleaner had previously been made redundant

during an earlier rationalisation at Harris Meats caused a "flash of insight":

I suddenly understood the importance of something I had known for four years: there had been ... earlier redundancies from the factory ... Did morbidity change with the threat of job loss two years before the factory closure? Yes, it did. But could I prove it?

At this point, by "good fortune", he met, socially, a medical statistician who was looking for part time work (Susan Nethercott) and "With enormous relief, I handed her the numbers to crunch". Next, Beale discovered the Joint Working Party on Unemployment and Health of the Royal College of General Practitioners and sought advice from Ian Russell, their statistical adviser. Russell queried the statistical technique that had been used. Now, using some money from the Science Foundation Board of the Royal College of GPs, Nethercott set about re-aggregating the data. This took up the month of October 1984. The paper was eventually submitted to the Journal of the Royal College of General Practitioners.

The first journalist to pick the study up was Andrew Veitch of The Guardian, who had also been the only one to write about the Regional Heart Study in 1982. Like Cook and his colleagues, Beale had made no effort to "subsidy" his work, and Veitch commented that full-time medical correspondents (in distinction to the experience of Dinwoodie and Christie of the Scotsman) on papers which could afford such things, *did* regularly scan journals, although the Journal of the Royal College of General Practitioners was one which he only looked at when he had "nothing else to read". Unlike the Regional Heart Study, however, and perhaps because the memory of the papers by Smee and Ramsden and Gravelle et. al. had faded by late 1985, the Calne study's impact expanded from coverage in the Guardian.

Beale described the media response and his own reaction to it: "countless interviews", his home full of film technicians, the telephone ringing incessantly. The culmination of all this was a summons from the BBC itself, and then yet more findings:

After the weekend, "Auntie" came, but, more important, Susan had found more significant results, the hub of another paper. To the addict, the fix is everything.

The next paper from the Calne study to receive media attention was

published on 22 December 1986 (Beale and Nethercott 1986d). It resulted once again in requests to appear on radio and TV. Andrew Veitch's fairly small piece in *The Guardian* was headlined "Redundancy 'affects health for years'". The new finding was that workers most affected by the period of "anticipation" were those with previous experience of unemployment. Those with previously stable work histories, however, suffered more AFTER redundancy. Veitch still feels that, even so

ministers have been reluctant to accept that the stress of unemployment has had a significant effect on the nation's health.

This account of the last major programme of research in the unemployment and health debate between 1975 and 1987 provides a vivid example of the presentation of "scientific discovery" as described by Latour (1987) and Woolgar (1982). Beale and Nethercott's findings were quoted and used in policy debate as widely as those of the LS, despite the fact that "referral" is a "softer" measure of effect than death. But from the point of view of the popular media the absence of dramatic endpoints such as heart attacks or suicides meant that rather less interest was shown. Beale and Nethercott's work was used more by writers of health manifestos for pressure groups or political parties. Neither did Beale and Nethercott ever become part of the "core group" in any informal sense -- there was no social contact between them and the members of the UHSG, or the LS team for example. For a general practitioner, to demonstrate that acceptable research could be done using existing records was of great importance, but within a rather different subprofessional entrepreneurial process, that of increasing the academic potential and therefore the status of general practice itself.

## **8.2 Stage Four (B): Metamorphosis**

### **8.2.1 "Occupationless Health"**

Because its author played something of a "midwife" role in the emergence of the new interests and alliances which now superseded the unemployment and health debate, it is appropriate to include in this section a discussion of Richard Smith's series in the *BMJ* on "Occupationless Health". This series of

articles ran between October 1985 and February 1986<sup>43</sup>. It is difficult to classify the series as part of the "academic" or "public" debates on unemployment and health. This difficulty is not accidental, but reflects the way in which the public debate was increasingly conducted without reference to the academic debate, and illustrates an important stage in the process by which knowledge and policy interact.

The series may be viewed as something which Smith did as part of the "cycle of credibility" followed by serious medical journalists. He had previously written series on alcohol and prison medicine, and followed "Occupationless Health" with one on research funding. The title of the series implies a "public" aim, and in content the articles were in the form of a series of reviews of both research and policy responses. The view of the Unemployment and Health Study Group was that having such a comprehensive review readily available, and the fact that it had appeared in such a "respectable" journal would add solidity to their case in policy debates.

Richard Smith was an Edinburgh graduate who had completed his "house jobs" and then worked and travelled in the Far East for some time. On returning to Britain, he had not gone back into clinical medicine but "fallen on his feet" into an assistant editorial post at the BMJ, which he made his career. He felt strongly on the question of unemployment, and, like other sympathetic journalists such as Bryan Christie and Robbie Dinwoodie, was disappointed at the lack of response to his work. The content of the articles will not be dealt with in detail here, as the research has been discussed in other chapters.

While planning the series, Smith approached Scott-Samuel, Cook and myself with a request that we read over each article before it was published to "check factual details". The group he chose as his "experts" might, therefore, be regarded as somewhat weighted towards the "Manifesto" end of the spectrum, but in the first paper, "Bitterness, Shame, Emptiness, Waste: An introduction to unemployment and health", he states:

the evidence linking unemployment with poor psychological health is much stronger than that linking it with poor physical health

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<sup>43</sup> The papers were eventually published in book form, Smith (1987b).

and

Studies on how unemployment affects physical health cannot match the sophistication of the psychological studies

What was “new” in this paper was Smith’s summing-up not of the outcome of research, but of the way research had been conducted:

Sadly, although unemployment began to increase dramatically in Britain more than five years ago, no study was ever set up to study specifically the effects of unemployment on mortality -- or indeed any other measure of health. Instead, clever use had to be made of data from studies set up for other purposes ... Some people to whom I spoke thought that the government had deliberately discouraged research on unemployment and health, because it did not want any data produced that might make continuing with present economic policies more difficult

Smith had “heard of at least two ... cases from England where applications ... had been rejected more on political than scientific grounds.” But, he goes on

others I spoke to subscribed less to this conspiracy theory and more to the idea that doctors’ leaders had been slow to wake up to the importance of unemployment to health (Smith 1985).

He also mentions the confusion of responsibilities between the Departments of Health and Employment, the attitude that there was no need to add “health” to all the other reasons for lobbying against unemployment, and the fact that “information is scattered through a variety of disciplines.” He thereby presents his account as a “balanced” appraisal of the mixture of reasons why, by October 1985, it was considered that “we knew little” of the effects of unemployment on physical health. The risk of being identifiable as his informants so alarmed Smith’s “expert group”, however, that they asked not to be acknowledged publicly for their role. Encouraged by the contact between himself and Smith occasioned by the series, in late November 1985, Scott-Samuel wrote to him to

suggest that it would be timely for the BMJ to publish an editorial on inequalities in health.

The justifications for this move were several. Firstly, the British government had, as a member of the World Health Authority, signed a document “Targets



for Health for All by the Year 2000", which committed it to reduce health inequalities (amongst many other objectives). Secondly, the BMA itself was now taking up the question of health inequalities in a number of ways, having established a working party to "look again at the Black Report". Thirdly, there had been recent advances in the technical sophistication of the available measures of inequality in life expectancy. The outcome of this was a paper by Scott-Samuel, entitled "Health Inequality -- back on the agenda?". Ironically, the editorial board of the BMJ turned it down. It was eventually published in the Lancet on May 10 1986 (Scott-Samuel 1986a).

### **8.2.2 Health Inequalities: back on the agenda**

The rest of the academic debate on unemployment and health in 1985–March 1987 was low key, and demonstrated a process of metamorphosis. As some had anticipated, the unemployment and health debate now began to be re-integrated into the older debate over the Black Report and health inequalities.

By early 1985, many academics knew the broad outline of the content of the forthcoming Decennial Supplement on Occupational Mortality to the 1981 Census (OPCS 1986). Many expected that what were seen as "increasing social divisions" would be accompanied by increased differences in the health of the various social classes. And this was what the Decennial Supplement seemed about to show. As early as March 1985, the BMA News Review carried a paper by Nicky Hart (the researcher on the original Black Report) documenting differences in life expectancy between the social classes, and urging GPs, in particular, to "play a vital role in pulling together the evidence of social and economic ills and thereby help put them on the health policymaking agenda". By 1 May of 1986, a table was circulating in "samizdat" fashion which showed the following changes in mortality rates by social class:

## Class Differentials in Mortality 1970-1983

### Standardised Mortality Ratios

Class	DS 1970-72	LS 1976-81	DS 1979-83	LS 1981-83
I	77	66	66	63
II	81	77	74	84
IIIIn	99	105	93	89
IIIm	106	96	103	94
IV	114	109	114	103
V	137	124	159	131

DS= Registrar General's Decennial Supplements on Occupational Mortality 1970-72 and 1979-83

LS= Longitudinal Study mortality ratios for 1% sample of the 1971 census who died in the years 1976-81, and 1979-83

Here it can be seen that the Decennial Supplements seem to be showing a quite striking increase in the "mortality gap" between the social classes, from 77/137 in 1970-72 to 66/159 in 1979-83. The LS figures show a smaller increase (Fox and Goldblatt 1986, Goldblatt 1986). The Decennial Supplement on Occupational Mortality for the period around the 1981 census<sup>44</sup>. was finally published on 29 July 1986.

At the same time, members of the LS team were also working on a "commission" from the DHSS to examine patterns of social mobility amongst members of their one percent sample of the 1971 census. The monograph was completed by August of 1986, though still unpublished by March of 1987, and must have influenced their thinking.

As repeatedly stated by the Registrar-General in the various information subsidies surrounding the publication of the Decennial Supplement to the 1981 Census, the LS had confirmed that

Marked changes in the shape and structure of the labour force took place between the 1971 and 1981 censuses

These changes did not affect the outcome that

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<sup>44</sup> for an explanation of the relationship between DS and LS see section 5.2.2.

Socio-economic disadvantage in 1971 would appear to be predictive of unemployment and ill-health in 1981 (Fox 1986).

Most of those unemployed in 1971, apart from those in the older age groups, were once again in employment ten years later. But those unemployed in 1971 had tended to be downwardly socially mobile. They were also more likely to have experienced marriage breakdown and movement from owner occupation to local authority housing. Furthermore, the children of those unemployed in 1971 were "disadvantaged" in terms of their risk of unemployment, the type of occupations they did obtain, and the type of housing they ended up in, regardless of the social class of their fathers. Fox expressed a fear that economic change was giving rise to an "underclass" of multiply disadvantaged families.

### **8.2.3 The economists' contributions**

In these later phases of the unemployment and health debate and the process by which it metamorphosed, economists made two important contributions. Only one of these was by an established participant in the original debate, but the tone and content of both contributions was entirely consistent with the position taken by economists throughout. The first of these to appear in public was "An investigation into the incidence and dynamic structure of sickness and unemployment in Britain 1965-75" by Wiji Narendranathan, Steve Nickell and David Metcalf of the Centre for Labour Economics at the LSE (where Jon Stern had been working when he wrote his own contribution to the debate). It was published in the *Journal of the Royal Statistical Society, (Series A)*, which appeared in early October of 1985). The content of this paper had been available as a working paper since 1982 (Narendranathan et. al 1982), and discussed by Jon Stern in his 1983 paper in *Population Studies*, (see section 6.2.1).

Like Stern in all his work prior to the "unemployment and health debate", these authors give prominence to the social concentration of BOTH unemployment AND sickness in the lower social classes and in certain regions and ethnic groups. Despite its potential importance in re-introducing the notion of the social concentration of both unemployment and ill-health into the debate, this paper received no media attention (not surprising in view of the

journal in which it appeared and the difficulty of the methods used) and was not commented on by activists, even for purposes of criticism.

In November of 1986, a paper entitled "Unemployment and health: some pitfalls for the unwary" was published in *Health Trends*, a lesser-known journal devoted mainly to health service issues, but refereed and regarded as "respectable". It was by an economist, Adam Wagstaff, who had recently completed a Ph D under the supervision of Prof Alan Williams at the Centre for Health Economics at York. He threw down a challenge at the beginning of his paper:

Contrary to what is often asserted ... the evidence regarding the impact of unemployment on health is far from clear-cut (Wagstaff 1986c).

He emphasizes the "complex methodological problems" involved and the fact that because of these complexities "it is frequently difficult for the non-specialist to evaluate the strengths and weaknesses of research reports about unemployment and health." He criticizes Moser et. al for not controlling for past health in their analysis and claims that

unemployment status at April 1971 will tend to act as a proxy ... for health status at April 1971

thus completely ignoring the argument that the effects of such selection on mortality should "wear off" over time. Like Narendranathan et. al.'s paper, Wagstaff's was noted by the Unemployment and Health Study Group, but had no media or political impact, at least not in the "public domain".

These examples of research findings and reports show the way in which the debate on unemployment and health turned towards a debate on class inequalities and the persistence therein of a "cultural-behavioral" versus "structural" dichotomy of approaches to these questions. Up to this point, the group involved with the Longitudinal Study had relied on the technical power of their argument for defence against those who were critical of "structural" explanations. In the next section, however, the strategy of the LS team began to change, and to shift in the direction of "enrolling" new allies from the transepistemic field in which the debate took place.

#### **8.2.4 Technical re-constitution: changing perspectives of the LS team**

In early January 1987, the next major paper from the OPCS Longitudinal Study was published in the BMJ. At least one of its author group felt that the message of their paper was ambiguous. It seemed, on one reading, to be a report of LOWER mortality amongst men seeking work in the week preceding the 1981 census (the 1981 "unemployed") during the period 1981-3 than had been found in the period 1971-73 for those "unemployed" in 1971.

The argument of the paper was that when comparing the effect of unemployment on mortality in the years following the 1971 Census with that in the years after 1981, three factors had to be taken into account.

1. unemployment had risen greatly, perhaps tending to make the unemployed a less "deviant" or "selected" group
2. the numbers of men "permanently sick" and "early retired" had also risen greatly, so that those still in the labour market even if unemployed might be more highly selected for GOOD health
3. the length of the average spell of unemployment was now much longer than in the early 1970s.

On the last point, a paper in the Employment Gazette for September 1986 (Hughes and Hutchinson 1986) had shown that the average number of weeks spent in unemployment in 1971 had been 8.4, in 1981 it was 20.5. These three factors might give rise to two opposing trends. The first two would tend to weaken any association of unemployment with mortality. The third might tend to strengthen the association. The results of the analysis duly demonstrated all three tendencies. The LS researchers adopted the strategy of comparing death rates for the years 1981-3 (all that was available at this time) with those for 1971-3. In 1971-73, the SMR for men of working age at death who were seeking work in Census week 1971 was 121 (adjusted for social class). In 1981-83 the equivalent figure was 112. Neither of these SMRs is significantly different from 100.

Broken down by age and by year of death, however, a different interpretation becomes possible, and this is the one adopted in the paper. For younger men (15-44 at death in 1971-3, 16-44 at death in 1981-3) the SMR is 162 in the earlier period and 160 in the later. These figures, because of the small number of deaths in the younger groups, are still not significantly greater than 100. But for older (aged 45-64) men, taking deaths in 1983, the SMR is

145, significantly above 100, and much higher than the SMR of 123 for men "seeking work" in 1971 who died aged 45–64 in 1973. The effect of selection by GOOD health into unemployment rather than permanent sickness is also more visible in the 1981–3 period. The SMRs for the older men being below 100 for the single years 1981 and 1982, but jumping to 145 in 1983, a classical "healthy worker effect" pattern, as the text points out. The authors conclude:

It therefore seems that for men at older working ages, the data for 1983 provide the most appropriate estimate we have of mortality among those who were seeking work in 1981.

Adding the 145 of older men in 1983 to the rate for younger men gave an overall SMR for this year of 147. Moser et. al. comment:

the standardized mortality ratio of 147 at ages 16–64 in 1983 is the best measure we have of overall mortality among men who were seeking work in our sample.

Thus, they nail their colours firmly to the mast of the "wearing off of selection effect". Without this argument, the figures could be interpreted as having demonstrated even less of a cause for concern in the 1980s than in the early 1970s. One co-author of the paper felt, therefore, that readers and potential publishers of the paper might well react "So what?"

Any claim to have demonstrated a cause for concern is based on the adoption of the 1983 figure the "true" effect, the one visible after the effects of health-selection into the "seeking work" category (as opposed to long-term sickness or early retirement) had "worn off". The paper also makes reference to Fox's unpublished work on social mobility and possible "residualisation" in the families of those men who had been unemployed in 1971 (see section 8.2.2.).

In a letter to the BMJ, Dr B S Smith writing from a Midlands district general hospital (B S Smith 1987), accused the LS team of having done no more than shown that "men who are at higher risk of ill health are more likely to be unemployed and more likely to die." Smoking, he argues, echoing Cook and the economic advisers (with whom he had no contact) could produce both the observed high rates of lung cancer and be indicative of

personal or personality problems which lead to alcohol abuse, drug dependence, and broken marriages ... Smokers are more likely to have road accidents, to have psychiatric problems, and

to have been in prison ... Because they continue to smoke, smokers would seem in general to possess less motivation ..

In their replies (Moser et. al 1987b & c), Moser and colleagues did NOT use the argument from selection effects wearing off, rather surprisingly. They argue that their men "seeking work" were selected for good health at the beginning of the study, that the great majority of the 1981 unemployed were in steady jobs in 1971 (according to Fox's unpublished monograph on social mobility), so that the 1981 unemployed can hardly be seen as some sort of deviant group. If the mortality of the larger, more socially heterogeneous unemployed group in 1981 so closely resembles that of the 1971 unemployed, how can both effects be due to predisposing factors? They conclude:

Reduction in cigarette smoking and changes in other aspects of lifestyle with adverse health effects are, of course, desirable. However, it would seem from available evidence that they are unlikely to be successful in removing health inequalities unless other problems associated with unemployment and poverty are also tackled. (letter by KA Moser, PO Goldblatt, AJ Fox, DR Jones, BMJ 21 Feb 1987, p. 509)

This letter is far less technical than any of this group's previous work on unemployment and health. It replies to Smith's claims about the moral characteristics of smokers and the unemployed in (at least partly) similarly "moral" terms. There seems to be nothing in the data they have presented on January 10th or elsewhere which would act as clear evidence AGAINST some kind of hypothesis to the effect that over the long term, "smokers" could be the ones worst affected by recession. The one possible technical argument, that the effect of smoking on a cohort, like that of chronic illness (or indeed "suicidal tendencies"), might be expected to "wear off" over time as smokers died, and that mortality in a group "selected for smoking" would therefore not exhibit the "healthy worker" pattern of **persistently** raised mortality levels, is not made here. Rather, these two letters make opposing claims of a more directly "political" kind -- should health inequalities be tackled by policies directed at the behaviour of individuals or by macro-economic policies bearing on poverty and unemployment?

The tone of this letter seems to indicate the beginning of a renewed effort by the LS team to "enrol" allies, perhaps of a new kind. Which made it all the more frustrating for them that the media response they expected to their

January 10 paper did not materialize . Only a single article appeared, in The Times of January 9 (Prentice 1987). There were no calls to City University requesting television or even radio interviews and no other coverage in national or local daily papers or weekly journals. More galling still, the Times piece was the result, not of the appearance of the paper in the BMJ, but of a feature-writer's search for "background" material for an article on a factory closure.

Other issues now preoccupied the science and health correspondents. And there was no "information subsidy" to tempt them. It must be remembered that the LS researchers "played the game according to the rules" and did not indulge in "soap box" exercises, a strategy which enhanced the credibility of their work in the eyes of colleagues. The appearance of the 1984 paper in the media had been due to an information subsidy organised by the UHSG. When I asked the member of the group who had co-ordinated the 1984 subsidy for his reaction to the media silence he remarked:

I would have told Andy Veitch that the excess mortality was 22% last time [that is, for 1971-73] and 45% this time, so he could say that unemployment had twice as much effect on mortality in 1981-83. By the way, that 145 was social class standardised, wasn't it .. [MB: I don't know] Oh well, but they [Moser et. al] do SAY that adjustment for class made little difference, so you COULD use it.

The question to be asked, therefore, by the researcher into the social problem process (as opposed to that asked by the dismayed participants) is why, on this occasion, unlike in 1984, the interest-groups who had once provided the information subsidy that carried Moser and colleagues' earlier paper into at least some degree of limelight did not behave the same way in 1987. The next chapter will explain why this was.



**CHAPTER 9**  
**UNEMPLOYMENT, PUBLIC HEALTH AND**  
**SOCIAL POLICY MARCH 1985 – MARCH 1987**

In chapters 7 and 8 it has been shown that one aspect of the development of the unemployment and health debate was a process of confrontation in which the arguments on both sides, but particularly on the side which held that there “was an effect” of unemployment on physical health, became more elaborate in both technical and substantive terms. In order to explain the fate of these developing knowledge-claims, this chapter will trace some of the processes which unfolded in the “trans-scientific field” in which the scientists were embedded. The “field” will be divided into four parts: (a) the pressure group and the Labour Party (b) the sub-profession (c) the media (d) the relationship between the government and the scientific community.

### **9.1 The Unemployment and Health Study Group: New Directions**

At its first meeting in 1985 (on 16 January), an agenda item was “Whither UHSG?”. It was felt that despite Kenneth Clarke’s statement at the MIND conference

We [are] not “over the top” yet as regards government attitudes, research findings and the Unemployment Health and Social Policy Report [the Leeds Report] ... the steadily more conclusive findings of research needed wider dissemination ... The facts about the quality of life on the dole ... and the effects of relative poverty of the long-term unemployed (e.g. on nutrition ...) needed wider dissemination.

And at the next Study Group meeting, on 19 February, Watkins and Scott-Samuel reported back on the CEPR Workshop

DHSS participants had acknowledged the causal implications but had stopped short of any firm commitment to action on the policy front

They felt that Gravelle’s discussion of Platt’s paper had been conducted “in incomprehensible econometric jargon -- an English translation was needed before any assessment was possible”.

The group's April meeting was in one way the end of an era, in that it was the last which was devoted mainly to the discussion of research into unemployment and health. There was bitter commentary on the CEPR meeting, and the write-up in the CEPR Bulletin. Several workshop participants had discussed the CEPR Bulletin's version and told Study Group members that they were

generally agreed that the report had been written in a tendentious way in order to convey the fake impression that there had been general agreement that more research was needed before any action could be taken on the health damage caused by unemployment. This would permit DHSS and government to claim that "something had been done", while continuing their inactivity over policy responses.

The new direction for the Study Group was signalled by a short paragraph in the April minutes about work by Gill Westcott on "The cost to the economy of maintaining unemployed workers". In June, she began a correspondence with Neil Fraser and Adrian Sinfield of Edinburgh University, whose paper "The Real Cost of Unemployment" had been published in March. The paper was circulated to all Study Group members in July 1985 for discussion at the August meeting. The Study Group began to take more interest in matters such as "job creation" from this point in time.

In June and August, the main topic at meetings was once again "Whither UHSG?". They felt that they had been successful in wringing TWO ministerial admissions that unemployment "affected health". The second was in the form of a letter (dated 1 July 1985) from Secretary of State for Health and Social Security Norman Fowler to Michael Meacher (who had, at the suggestion of UHSG members, sent Fowler a copy of the Leeds Report). In the letter, Fowler stated that he

would not question that unemployment may well have negative effects on health in many cases

and that the DHSS

continues to monitor research evidence on the relationship between unemployment and health, to take it into account in the formulation of policy, and bring it to the attention of other government departments as appropriate.

At the UHSG meeting of 8 August, when this letter was discussed, Westcott suggested that this was the moment for the group to “broaden its focus from unemployment to unhealthy aspects of economic growth and the social costs of recession.” Having offered this “new” perspective (though see section 4.3.3), and the skills necessary to pursue it, Westcott then announced her resignation from the group for personal reasons. Another meeting did not take place until November. By this time, a new health economist (Catriona Waddington) had been recruited. The continuing question “Whither UHSG?” still appeared on the agenda for meetings. As part of these explorations, Jennie Popay was invited to address the group in February of 1986, on her research on how social workers, health visitors and health educators dealt with unemployed clients. This new direction, in the direction of “alleviation of the effects of unemployment on health” (as advocated by the government economist in section 7.4), seemed to mark an upturn in interest in the Study Group. The meeting of 21 February 1986 was attended by 13 people. It seems that at this point some participants from the beginning of the debate, who had stood back from the intensely technical developments of 1982–84, now re-entered it, and aided to complete the shift from “research dissemination” to “service” and “policy” issues.

On 2 May, 1986, came the first public indication that the Labour party intended to take up the health consequences of unemployment as one reason for its own economic strategy. A House of Commons press release by Meacher stated:

Over 17000 people have died as a result of unemployment since 1979, Michael Meacher, Labour’s chief spokesman on Health and Social Services, reveals today at a North-Western TUC Health Committee

Using Scott-Samuel’s by now well-publicised extrapolation of the 1984 LS paper, Meacher claimed that “Just over 3000 people will die this year because of unemployment”. The press release continued, also using the results of Beale and Nethercott’s study:

The list is endless. The impact on the quality of life for the unemployed and their families is devastating, The cost of treating unemployment related disorders is astronomical, and soaring, while health budgets are standing still at best, and are often cut.

This links the unemployment–health issue to the question of “health service expenditure”, on which Labour spokespersons had been scoring regular victories in Parliament and the media ever since the Radical Statistics Group’s “Unsafe in The Hands” pamphlet of Easter 1985 (see Section 9.2 below).

The inexorable rise in unemployment engineered by this Government clearly indicates the need for a massive increase in health spending and yet health service budgets in the 1980s have at best stood still...

The press release also uses Fraser and Sinfield’s work on the costs of unemployment.

On 17 May, the BMJ’s regular “Letter from Westminster” column was headed “Unemployment Kills, claims Labour’s Michael Meacher”, validating Meacher’s figure of “3000 deaths” in 1986 as “a statistic based on data provided by the OPCS Longitudinal Study survey of the effects of unemployment.” The Westminster columnist, Philip Johnston, comments

Statistical experts and medical researchers may be able to find major flaws in Mr Meacher’s analysis. But to the layman -- namely the ordinary voter -- it is a chilling catalogue. The claims appear to be outlandish, but even if only half true they can hardly be ignored

At the end of June, the Labour Party launched the first of a series of regional campaigns in Newcastle upon Tyne, “The Battle for Jobs and Welfare”, stressing the health and social costs of unemployment. David Hencke, Westminster correspondent of the Guardian, found that the “attempt” to use OPCS LS figures “to claim that 1294 people have died in the North as a direct result of unemployment since 1979” was “the most controversial part of the campaign”. The headline to his piece was “Labour seeks to blame deaths on unemployment”. Here again, the unemployment and the NHS funding issues are explicitly linked.

By the UHSG’s July meeting, the Labour party had also produced “Health for All – a charter for preventive health”, which was welcomed by “Manifesto”-oriented doctors. The Study Group was now set on a path which combined macro-economic discussion with “service issues”. In September, the Leeds Report itself was re-issued, having been reprinted by the Manchester University Department of Community Medicine Centre for Professional Development.

In October, a research and pressure group which had been set up in April 1985, the Employment Institute ventured briefly onto the territory of the unemployment and health debate in its "Charter for Jobs – Economic Report vol 2 no 1". Joanna Munro and Alan Shipman (both young economists) had produced a report entitled "The human costs of unemployment" which drew on discussions which Munro and Shipman had held with members of the UHSG. They had by no means swallowed the line of the Study group, however:

Statistical association on its own [they wrote] is no proof that unemployment is a cause of ill health. For it may be due to the fact that people in poor health are more likely to become unemployed and have greater difficulty in finding work again once they have lost their jobs.

This quote shows just how far Moser and her colleagues' concept of the "unhealthy nonworker effect" was from penetrating the policy debate, even TWO YEARS after the publication of their first paper in the Lancet. Munro and Shipman go on to discuss possible "mechanisms" by which unemployment may affect physical health, such as the physical demands of the sort of work done by people more at risk of unemployment, and the effect of poverty on nutrition, heating and access to medical care. As in the rest of the debate, the effect of unemployment on mental health (short of suicidal behaviour) is not questioned. On suicide and parasuicide they write:

The form of the association between unemployment, suicide, and parasuicide, is not incontrovertible. (Munro and Shipman 1986)

though they quote Steve Platt, as well as an "informal account" from Social Work Today of the suicide of a young man in Shotton in support of the possible "causal" effect of unemployment on suicidal behaviour.

A member of the LS team attended a conference on unemployment and health arranged jointly by the UHSG and the Liverpool-based Social Research in Health group, at Manchester Town Hall in November 1986. This was the only time that any member of the group of researchers at the SSRU working on the LS data ever attended a meeting organised by the Study Group. He was greatly intrigued by the Study Group members he met, but considered the tenor of the conference "unworldly", "utopian" and not seriously connected with the epidemiological work on unemployment and mortality in which he was involved. But these "utopian" topics were working very well for the Study Group, which went into 1987 with unprecedented support. Members continued

to be invited to speak at meetings all over Britain, to advise on research programmes (including the ESRC's "16-19 Initiative") and talk to journalists (but now to feature writers more than "news" reporters). This had been achieved without members gaining an understanding of "health selection effects", or making any specific use of the more technically sophisticated arguments developed by either the medical statisticians or the economists.

## **9.2 The political agenda and the "New Public Health"**

Two things are illustrated by the above account of the activities of the Unemployment and Health Study Group. One is that by mid-1986 it was no longer felt to be necessary for members of the group, or for any other activists, to use "health" considerations in order to get unemployment onto the Labour Party's agenda or to use "unemployment" to raise the profile of health issues. The other is what happens when a pressure group seems to have gone as far as it can in one direction (extracting confessions from Ministers), and sets out in another. The saliency of "research" to the group was greatly reduced as it took up a new set of issues around the "alleviation" of unemployment and of its effects, and attracted interest from health educators and other paraprofessionals. Another process which unfolded during the year 1985-86 was to reduce the importance of the research on unemployment and health to "Manifesto" community medicine. In this process, "Social Medicine" and "Public Health" became more prominent items on the agenda of Community Medicine, and even of the wider profession, carried there by a number of different issues. Unemployment and health was itself beginning to become redundant.

In May 1985, the Labour party took up a lively offensive on NHS expenditure after the government had circulated a free pamphlet entitled "The Health Service in England" claiming unprecedented rises in expenditure on, and provision of, health care during the lifetime of the Thatcher administration. This offensive was greatly helped by a counter-information subsidy produced by the Radical Statistics Health Group called "Unsafe in Their Hands". The author group included Derek Cook. It was published and press-released on 8 April, accusing the government of presenting "misleading and dishonest figures" ... which did not allow for the rate of inflation specific to NHS costs,

the rising number of elderly people, and the expense of new medical techniques. "Unsafe in Their Hands" was much quoted over the following two years, for example, in the Opposition Day debate on the NHS of 2 July 1985 (Hansard cols 203–241). Michael Meacher renamed the government leaflet "Fowler's howlers", and pointed out that treating a larger number of patients, as the leaflet claimed the NHS was now doing, was not necessarily a positive indicator of the nation's health. On the contrary, it

may merely reflect the fact that more people are ill under this government ... the trebling of unemployment, the halving of housing investment, the doubling of poverty, the weakening of controls over health and safety at work ... one does not have to be a statistician [!] to know that things are going badly in the NHS (col 204)

During the debate, Labour health spokesperson Frank Dobson was not contradicted when he declared

For a long time the government denied that unemployment leads to ill health. They cannot deny it any longer. Reputable surveys show that general practitioners face higher demands for primary health care from people who are out of work ... death rates among unemployed men can be over 20 per cent [sic], that para-suicide is nine times more likely among the unemployed.

Neither the medical profession as a whole nor any of its subgroups took up the potential threats to health of the proposals for the reform of social security, which were being discussed in the Cabinet during the spring and summer of 1985, and were issued as a Green Paper on July 3. However, the unease which was aroused by some of the proposed changes fed into a revived medical and public health debate on "growing inequalities".

In February 1986, Frank Dobson addressed a meeting of the Progressive Strategies for Health group in Sheffield, and told the audience of 150 that NHS funding was a "trivial" issue in comparison to poverty and unemployment, thus bringing Labour Party expressed views into line with the professional ideology of "Manifesto" community medicine. Even so, Dobson came under fire for failure to commit his party to more specific health policy measures. Scott-Samuel asked him why Labour did not:

sit down and decide what is feasible and commit itself to making people live longer by 'x' number of years

Others called for a salaried dental service, for health authority members to be elected, for more redistribution of resources to under-funded regions. Dobson insisted that he and Meacher had been attempting to "force the debate on the government's handling of the NHS into one about the nation's health rather than whether or not NHS resources had increased by 0.05 percent" (Feinmann 1986). Peter Draper called for the Labour party to "adopt a new approach to public health" and

get across ... that it is the present rampant, free market ideology that is the health hazard.

On 26 March the H&SSJ published an article written jointly by Draper and Scott-Samuel on "Whatever happened to Public Health?" (Draper and Scott-Samuel 1986). The social problem claims made in this article range from "crumbling sewers and death from hospital acquired food poisoning" to the arms race. It sets out an agenda for a new "public health militancy". Following a serious outbreak of salmonella poisoning at Stanley Royd hospital in Wakefield, the Secretary of State for Health and Social Services, Norman Fowler, had ordered an inquiry into the role of "public health doctors and community medicine", announced in the Commons on 20th January (Hencke 1986). Draper and Scott-Samuel used this opportunity to gain publicity for the ideas of "Manifesto" community medicine. They called for a greater understanding of

the health significance of public policy more generally, for instance, in agriculture or housing, in order to develop health enhancing rather than health damaging public policy

Also, echoing Frank Dobson, they claimed that

we [now] have the widespread dissemination of an economic philosophy that is intrinsically in conflict with a public health perspective ... The conflict between free market ideology and public health is one of the reasons why a public health approach is intrinsically "political" <sup>45</sup>.

Copies of the H&SSJ article were circulated to a group of people who were then invited to begin discussions aimed at setting up a "Public Health Alliance".

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<sup>45</sup> It should perhaps be noted here, however, that in the winter of 1987, the Public Health Alliance which emerged from these efforts declared itself firmly "non-political" and "non-aligned".



Meanwhile, the inquiry into public health (christened the Acheson inquiry, after its chair, Dr Donald Acheson, the Chief Medical Officer at the DHSS) was calling for evidence from various organisations and individuals. Its terms of reference included

determining the health needs of whole populations, evaluations of the outcome of services, medical advice and support to local authorities (letter circulated to all members of the Society for Social Medicine, 18 April 1986)

On 19 April, the *Lancet*'s "Commentary from Westminster" column claimed that the state of the NHS was the issue which "most disturbs" the world outside of Westminster. The by-election campaigns in Brecon and Radnor and in Fulham had, the columnist David McKie feels, demonstrated this clearly. Opinion polls of various sorts had shown that the public were worried about the state of the health service and disapproving of the Conservatives' handling of it (McKie 1986a).

In early June, the BMA's Board of Science and Education began to circulate interested groups and individuals (including Radical Community Medicine and the Radical Statistics Health Group) asking for written evidence to its "Working Group on Deprivation and Ill-Health"<sup>46</sup>. On July 1st, not to be outdone the Faculty of Community Medicine published a "Charter for Action on Health for All by the Year 2000". This document called for the government to declare that

the health of its people is one of the most important products of a civilised community ... [and to] consider the health consequences of legislation and policies in all government departments ... pursue policies designed to eliminate absolute and relative poverty ...[and] to reduce inequality

Alwyn Smith, retiring as President of the Faculty of Community Medicine, told the *Guardian*:

Britain was the leader in public health a generation ago. It has now lost that position ... and the result is that we are falling behind our neighbours in those areas -- child health and immunisation, health promotion and prevention -- when we should be in front.

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<sup>46</sup> For the eventual fate of this exercise see *British Medical Journal* 1987.

Here he explicitly links the management of infectious disease ("immunisation") with "prevention and health promotion", indicating an enrolment strategy now possibly opening to the subprofession and its Faculty in a period when AIDS and health inequalities were prominent items on the agenda of public debate. David McKie's "Commentary from Westminster" column pointed out the good use to which the Labour party put this document. To them, he felt, it was

No surprise ... but a clear political bonus. It allows them [Labour] to relaunch their standard allegations quoting such a distinguished and impartial umpire as the President of the Faculty, Prof Alwyn Smith, ... (McKie 1986b)

The production and dissemination of this document may be taken as a sign of the strengthening of the position of the "Manifesto" position within "the Community Medicine establishment". By the spring of 1987, for example, "rising young stars" on the more "managerial" side of the subprofession were telling me they "took it for granted" that questions of inequality were just as central to "health policy" as medical audit and performance indicators.

On 29 July 1986 the Registrar General's Decennial Supplement on Occupational Mortality was published, to be followed by a series of articles in the "quality" media on social and health inequality. In September, the appearance of a monograph by Peter Townsend and colleagues on "Health Inequality in the North-East" kept the issue in the public eye. In response to this, a relatively new junior Health Minister, Mrs Edwina Currie, claimed in a widely reported speech that, in the words of one article:

Poor health among people in the North was due more to ignorance than poverty or unemployment

This speech, and various replies to it, penetrated the popular press, with headlines such as **Eat Your Words, Mrs Currie, and Bananas! MPs Blast Edwina** (Daily Mirror, September 25). Badges and stickers were quickly produced with a **Currie and Chips** logo, to be distributed by groups such as the London Food Commission (some of these found their way into Departments of Community Medicine). This ministerial initiative was widely regarded as an example of an ambitious "new girl" taking it upon herself to counter the beginnings of a consensus amongst a variety of professional and pressure groups that aspects of social and economic policy could have implications for

health. That is, it was seen by observers as a move in the "cycle of credibility" of a junior Minister, entailing the necessity to balance the costs of "sensationalism" against those of anonymity. The ensuing debate on the diet of Northerners was, however, only sporadic.

### **9.3 The state and the media: the "problem of suppression of information"**

The media attitude to the delay in publication of the Registrar General's Decennial Supplement on Occupational Mortality is consistent with what had been the media response to "unemployment and health" throughout 1986. This, in turn, reflected the growing fertility for journalists of the disillusion in the civil service with government attitudes towards "social engineering", and towards the legal position of civil servants in relation to the Official Secrets Act. The whole of this story cannot be traced here, although the use of "information subsidy" by disgruntled users of "official data" has been a recurring theme in the present study. But throughout 1986, whenever "unemployment and health" appeared in newspapers and weekly journals, it tended to be linked to the "problem" of "government suppression of information".

Early in 1986, Bryan Christie gave me one journalist's point of view on why "suppression of information" was now a more interesting topic than "Death on the dole". He commented:

I wouldn't write anything more about unemployment and health now. It's quite clear that unemployment does harm health. The real story now is how civil servants are sitting on information.

His generalisation of "civil servants" is somewhat too broad, as the reason why journalists BECAME aware of "suppression" was that SOME civil servants (especially but not only professional advisers such as planners and research managers) were occasionally producing "leaked" information. The frustration of health planning advisers in Scotland, for example, is expressed in a feature on the Scottish Health Services Planning Council which Bryan Christie wrote for the Scotsman in October 1985. The SHPC's Secretary was Mr T. Drummond Hunter. Hunter told Christie that the "career" civil servants:

regarded strategic planners with ill-concealed disdain ... Why turn over stones that did not need to be turned over? Why open cans of worms that did not actually need to be opened

Interestingly, here Hunter speaks of the struggle by policy planners for "occupational survival" as outlined by Prince in exactly the same words (see section 3.3). He felt that as a Quango, the Planning Council (set up in 1974) had fallen into disrepute during the Conservative administration. It was instructed to confine itself to devising economy measures. The fate of the Council, as told here, sounds remarkably similar to the problems facing the Central policy Review Staff as related by Heclo and Wildavsky.

The issue of "government advisers being ignored" was also taken up in relation to the review of social security. On 11 November 1985, Michael Meacher in the Commons debate on the Queen's Speech, accused (as Hunter had done) ministers of a "bunker mentality". He claimed that "advice was now tailored to what ministers wanted to hear" (Travis 1985), and that even "top civil servants who do not share ministers' political prejudices had been sidelined .... objective and independent advice no longer reached ministers".

In the same vein, on 28 February 1986 Jeremy Laurence wrote for New Society on "How Ministers fiddle figures" (Laurence 1986a), enumerating the "distortions of statistics on public spending, the unemployment rate, NHS spending, tax relief to home owners, hypothermia deaths and the health of the unemployed". The Central Statistical Office, he wrote

continually finds itself in the position of having to defend its integrity ... Statistics are the bedrock of government. They supply the facts against which its policies are justified and the measure by which their success (or failure) is assessed.

Thus Laurence delineates one of the new "social problem processes" in which government statisticians had, especially since the 1980 Rayner Review, found themselves participating (see section 3.1). It was this process, and the claims and information subsidies which arose from it, which gave some of their shape to the alliances which now began to form around questions of public health in 1985 and 1986. A "conspiracy theory", Laurence felt, was, in this case,

not mere fantasy. Statisticians fight regular battles with civil servants and ministers over the presentation of figures.

Sir John Boreham, who had retired as head of the Central Statistical Office in

1985, told Laurence:

Governments are composed of human beings. They don't like publishing statistics that show their policies aren't working -- yet. That's when it gets interesting.

According to Laurence, the statisticians felt that they "nearly always won". An exception had been the removal of a table on the health of the unemployed from the 1986 edition of Social Trends. Boreham himself had "got as far as drafting his resignation letter once" and was aware that "technical questions such as the recognition of hypothermia as a cause of death ... get fogged up with political questions." Overall, however, Boreham considered that

"The government is pretty clean in this country -- BECAUSE WE HAVE A BLOODY POWERFUL SCIENTIFIC ESTABLISHMENT (my emphasis, p.362)

Here can be seen the very obverse of the complaints voiced by administrators who spoke to me that "statisticians would not do quick-and-dirties" and "stood on their professional pride". Seen from the statisticians' point of view, their obduracy was part of what kept government in Britain "clean" (that is, "keeping government honest" was being offered to the media as a "social problem" to which professional statisticians' skills and ethos was offered as "the answer").

On 2 June 1986, however, the government began a "major review" of the work of the Office of Population Censuses and Surveys, to be carried out by the Machinery of Government division of the Cabinet Office. No warning of this review was given to top officials at the OPCS before the public announcement. The assistant secretary of the First Division Association, the trade union to which many civil servants working for OPCS belong, told Richard Norton-Taylor of the Guardian that this had

"major implications for the integrity of government statistics..."

The response of "some officials" was that the proposal to devolve statistical work to the appropriate government departments would produce a temptation to "doctor statistics". Another proposal was that interviewing surveys such as the General Household Survey should be carried out by private research organisations. Norton-Taylor reminds Guardian readers that

The government has been reducing the supply of official statistics and the money spent on collecting them since a Rayner review carried out in 1981. It stopped work by the Central Statistical Office on the distribution of wealth and reduced the number of official estimates on income distribution.

## 9.4 The future of the debate?

On 21 March, Laurence wrote a two-page feature on unemployment and health. He cites Warr and colleagues' research as having "proved" that mental health was damaged by unemployment, but

For politicians ... a mental patient is less persuasive than a corpse.

and goes on to quote the LS (referring in a short phrase to the "unhealthy nonworker effect") and Steve Platt's and Norman Kreitman's research on parasuicide. The latter, he feels, shows that "The link between suicide and unemployment is one of the strongest so far established" despite Gravelle's criticisms, which had still not been published at this time. Laurence calls for

more and better research ... to keep up pressure on the government ... [which is] clearly embarrassed by the research.

However, this account of the unemployment and health debate has indicated that it is not the mere *existence* of research findings, or even the opinion of the academic community as to their "quality" which ensures the entry of knowledge claims into the public sphere and policy debate. In order to become *either* "true" *or* "policy-relevant", knowledge claims need to be "points of passage". That is, they must attract the assent of a number of groups, a network which will pick up and pass on the claims intact to a wider audience. What has been described in this chapter is the dispersal of the actor-network that carried claims about the health of the unemployed. The network had, in any case, never been strong enough nor sufficiently well connected to the groups of researchers, to ensure a very wide or consistent acceptance of the link between unemployment and ill health as a "fact". But it did not disperse solely because of its weaknesses or because of the strength of its opponents (in government or in "rival" groups). An important factor in the final detachment of the network from the producers of new knowledge claims was the *success* of constituent groups in attaining their objectives in other ways, and through

other alliances which did not include the researchers.

By mid-1986, the unemployment-health link was sufficiently well established amongst non-expert opinion for new initiatives to have been undertaken whereby local authorities and health authorities in some areas set up programmes of job-creation, aiming their programmes especially at groups hardest hit by recession (see Harris and Smith 1987). There was a new task for health education and health promotion professionals, at least, as much as this relatively small group could deal with at the time, in providing special measures to protect the health of the unemployed (Laughlin and Black 1985). "Unemployment and health" was accepted as a topic for consideration by the new and expanding "'Healthy Cities" initiative, the most conspicuous aspect of the WHO Health for All by the Year 2000 programme, at least in Britain. There was also sufficient residual academic interest for researchers to find space in journals for occasional papers on the subject, including many participants in the earliest stages of the debate getting some "return" on that investment (Westcott 1987, Brenner 1987a, 1987b, Forbes and Macgregor 1987 Charlton et. al 1987). These groups (health educators and "promoters", community development workers, academics) could continue as if "the health of the unemployed" had been accepted as a legitimate problem requiring the use of their skills. Just as the health educators and community development workers could ignore the doubts and complexities of the academic debate, so could researchers write as if the "wearing off of selection" argument had never been proposed. For example, Forbes and McGregor (who also must be regarded as *politically* relatively sympathetic to the aims of those who promoted Brenner's work) wrote in 1987:

Interpretation [of the LS results] is difficult because the researchers *cannot control for the role of ill-health in generating some of the unemployment experienced by their sample*

and Charlton (a statistician, by this time working for the DHSS and in terms of political background and allegiance far less sympathetic to Brenner's early sympathisers):

... [LS] findings ... do not rule out the possibility of a selection effect...

Other groups now discovered new paths towards their goals and new markets for their expertise. A wish to change the Labour party's policies

towards unemployment had been one aim of early participants in the debate (see Chapter 4.). This now seemed so out of date as to be almost bizarre. Events between the General Elections of 1979 and 1987 had nearly wiped out the memory of Dennis Healey's deflationary economic policies, and of the overriding concern with inflation rather than unemployment, which had meant that special effort was seen as necessary by some Labour Party activists in the late 1970s to place unemployment higher on the party's agenda. Those who wished to increase Labour's concern with health had a similar experience. "Health" was second only to "unemployment" in Labour's stated priorities in the 1987 election, although the two issues were seldom explicitly linked.

Meanwhile, "Manifesto" community medicine and its allies in academic departments found other social problems to tackle: health inequality, democracy in the NHS, equity in resource allocation, the need for more public health education at a time when the threat of AIDS seemed to loom over Britain's sexually active population. All these concerns could be combined, conceptually and organisationally, under the banner of the need for a New Public Health, raised initially by Peter Draper, supported by Scott-Samuel, Player, the editorial group of Radical Community Medicine, and even sections of the BMA, as illustrated by two editorials in the BMJ, both written by Richard Smith (Smith 1986a and b)<sup>47</sup> Other enterprising professional groups could also "get mileage out of" the new social-problem processes which succeeded "unemployment and health": the first Chair of the newly formed Public Health Alliance was the Secretary of the Health Visitors' Association, Shirley Goodwin, and the first major organisation to affiliate was the Royal College of Nursing.

On the subject of new directions for community medicine, the editorial group of Radical Community Medicine, in the Autumn edition of 1986 (vol 27) could see that: "public health" was "beginning to be fashionable again ... the Black Report is probably more relevant here than the Acheson inquiry [on the organisation of Community Medicine] looks likely to be." Here the "Manifesto" group more or less explicitly pose the social problem of "health inequality" as a

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<sup>47</sup> Smith's participation in the debate is a good example of "entrepreneurialism" in its later stages, and the heterogeneous nature of "network-building" in the translation of "science" into public and policy debate. His series on "Occupationless Health" and the resulting book was not an original piece of research. It took pains to be "fair" but did not avoid controversy. It made no contribution to the "scientific debate" directly, and yet was as influential as any other piece of work in maintaining "interest". It also played a role in Smith becoming a favoured candidate for editorship of the Journal.



likely saviour of their mainstream subprofession from the threat of being re-organised out of existence. The editorial group now set about planning a whole issue on "Health Inequalities" for 1987.

During the summer of 1986, the vehemence of both the claims being made by the new studies of health inequality and the government's attempts to counteract them was very similar to the mood of the unemployment and health debate in 1981. John Fox, as director of the Social Statistics Research Unit at City University, within which the LS team were located, had been centrally involved in both the unemployment and health debate and the debate on health inequalities. The reason why so much of the work on the LS looked (to me, and to other participants from "Manifesto" community medicine and the "New Public Health") so "good" and "relevant" was that at one point it almost did provide an "obligatory point of passage" for two major debates: on the health consequences of unemployment, and those of other social inequalities. The concept of the wearing-off of selection effects was explicitly presented by SSRU staff in various papers as an answer to problems in both these areas of inquiry. It seemed to hold the potential to close the circuit and finally bring into contact the concerns expressed, for example: at the Stirling "consultation" of 1982; in the work of some economic advisers since 1979; and in the Black report, with those of campaigners on the "single issue" of unemployment and health.

Some participants in and observers of the debate had suggested that the "excessive" nature and "showy" presentation of Brenner's early papers had actually damaged the cause of those who wished to carry out a "serious" investigation of whether or not unemployment affected health. If the debate had been conducted from the beginning in a lower key, if claims had been put forward "according to the rules" of academic scepticism, then the attention of hostile forces would not have been attracted to intellectually fragile work. There may be some substance in this argument, which only further studies of other similar debates would clarify. However, workers in the "strong programme" of SSK (for example, Shapin 1982) have suggested that in some cases it IS the politically sensitive nature of debates which produces a more and more intricate and technical form in knowledge-claims, as a response to the strenuousness of the challenges coming from opponents. This is the process which I have called "technical fragmentation". Shapin has suggested that

the social interests of groups of scientists should not be seen as sources of error and distortion

but, on the contrary,

the action of conflicting social interests ... may be seen as an important element in the development of bodies of knowledge valued as 'interest-free' ... (p. 143)

One way of looking at the outcome of Fox and his colleagues' necessarily careful (partly because of Brenner's "errors" and partly, perhaps because of the very hostility of government which many bemoaned) defence of their position is to see it, as many participants did (and not only those who "agreed" with the basic proposition that unemployment damaged health) as a major piece of intellectual virtuosity. The superiority of Moser et. al.'s work over that of Brenner was never questioned. There is, therefore, no explanation to be found here for its lack of persistent impact on policy debate.

It is here that the work of the "strong programme" of the sociology of scientific knowledge, and its development in the direction of a "translation theory" seems a useful aid to understanding the progress of this debate. Technically and ethically (in that they, too, "played the game according to the rules") the work of the LS team was not seriously faulted. But their alliances were, in Latour's terms, just not strong enough. It was as a consequence and not a cause of the weakness of the "lash-up", the institutional nexus between researchers, pressure groups, professional subgroups and the Health Departments that the question "does unemployment cause ill-health?" remained "scientifically" an open one at the end of my period of observation. The social-problem processes in which the non-academic participant groups invested their energies, and from which they hoped to gain increased prestige and command over resources, could now simply move on, without anyone having to be concerned with this question. Finding themselves thus stranded, the academics, too, had to "move on", according to their own unwritten rules of reasonable and competent performance (see Chapter 1). Specifically, during 1986 and 1987, the field of contestation between health economics and community medicine shifted from unemployment and health to health inequalities, where a sharp public controversy (Illsley and le Grand 1986, Illsley 1987, Wilkinson 1986, Scott-Samuel 1986b) was followed by a move towards reconciliation and mutual enrolment (le Grand 1987) between different groups

of experts, in the face of continued government pressure being exerted on ALL forms of "social engineering" (see section 6.2.1.), including "economic advice".

We have seen that when a major paper from the LS team on the health of men seeking work in the week of the 1981 Census was published in January of 1987, it received no publicity at all (section 8.2.3). Three weeks later, the Medical Research Council told Fox that his request for a new programme grant of 5 years had been cut to two. Shortly afterwards, the team were told that due to the large pay rise granted academic staff, it was doubtful whether even two more years work could be funded by the MRC. (In the event, they did receive two years' funding). It is tempting here (and was even more tempting to me at the end of my period of observation, still closely tied-up with my own fieldwork identity of "apprentice") to regret that the debate went no further, that there was no confrontation between the "behaviorist" interpretations of Gravelle and Cook and the "structuralist" ones of Moser, Fox and their colleagues. The purpose of this account has not been to make recommendations about future research programmes on unemployment and other forms of inequality on health, although the description of those alliances which did *not* become consolidated, such as those between health and poverty researchers, may appear suggestive. But perhaps it should not surprise us that when an account of scientific controversy is written so as to make explicit, rather than, (as normally happens), to hide the relationships, accidents, and decisions which socially organise the debate, alternative directions seem to appear which, to some, could prove rather "interesting".

## CHAPTER 10

### CONCLUSION: THE SOCIAL-PROBLEM PROCESS AND THE ABANDONMENT OF KNOWLEDGE

This account of the unemployment and health debate between 1975 and 1987 has tried to show the ways in which entrepreneurial activities by professional and disciplinary subgroups produced knowledge claims and attempted to establish these claims as resources ("obligatory points of passage", see section 2.1) to other significant groups in their "trans-scientific fields". It has traced the formulation of the claims, and the controversy to which they gave rise, by treating the debate as a social problem process, and (for the latter part of the period) following it "in hot blood". The aim has not been to make judgement on the "correctness" or "incorrectness" of the claims, but to trace the ways in which groups in the scientific and trans-scientific environment of the researchers adopted or opposed knowledge claims, or took up a "wait and see" position, all as part of their own occupational and micro-political strategies.

#### **10.1 A twilight world**

At the beginning of the process, the scientific groups involved were:

- statisticians working on "public health" issues, influenced or trained in the traditions of the General Register Office who regarded vital statistics as a form of social monitoring, and feared the decline of this role after the Rayner reviews
- economists involved in work on either health or labour market issues, with links to a government advisory role which had been less threatened than that of the statisticians by the political and administrative changes which took place during the period under study
- medical sociologists engaged in the more "quantitative" types of study, but much further removed from any advisory role to departments of state.

All three of these groups sought a more prominent role in the health planning process, at a time when health planning questions were high on government agendas. Their participation in the unemployment and health debate can be understood by setting it against the kinds of claims they made about "public health" and health "service" issues, within the economic, political and administrative context of the period at which the debate took place. Those

disciplines whose members claimed that unemployment *did* affect health were the more threatened ones (medical sociology, "social/medical" statistics) and the professional group with whom they allied was similarly threatened, in its turn, not by Rayner reviews or other cuts in government funding provided for research, but by successive rounds of NHS reorganisation. Economists, in contrast, had relatively well established alliances with policy-makers in government departments. Some were also quietly pursuing a long-term strategy of "inter-esting" factions within the medical profession (see Mulkay et. al. 1987a and b). Others offered interpretations of the relationship between unemployment and physical health as "resources" to client groups within government with the skill for which they were respected -- the ability to tell whether the customer wanted "the Black Magic or the Dairy Box". In 1979 this involved questions about "whether the RAWP formula should be changed to include measures of unemployment". In 1981 it included a critique of Brenner's methods. In 1984 it was a matter of "recommending measures of alleviation" within an accepted context of high rates of long-term unemployment in particular. Economists made no efforts to gain media attention or to engage in pressure-group activity, and were indeed acutely embarrassed on the rare occasions on which the media did feature their work (a headline claiming "No Jobs Link with Ill Health" was dismissed by one economist as "What I am SUPPOSED to have said", see Black 1981). The statisticians, in contrast, and also true to their "form" as seen by civil servants, pursued "the truth" (which, as one remarked bitterly in late 1987, "you are only allowed to tell *this* government once in a career"). Thus did different occupational cultures affect the ways in which members of the two disciplines reacted to changes in macro-economic and political climate.

The claim "unemployment causes ill-health/mortality" was brought to public and political debate by the activity of an "intermediate body" (in Hall et. al.'s sense) whose role was to promote informed discussion of health policy issues, and two quasi-pressure groups (one terming itself a "study group", the other a "forum") which provided "subsidized" information, not only to the media, but to other entrepreneurial professionalising groups (health visitors, health educators, community workers). Yet we must remember that the membership of the Unit for the Study of Health Policy, the Unemployment and Health Study Group, and the Social Costs of Unemployment Forum was also largely COMPOSED of members of the major subdisciplines and subprofessions (sociology, community

medicine, health education, community health development, etc). Participation in the pressure groups was one strategy by which the researchers stimulated demand for their expertise, and the professionals for their authority.

Nor was it the WHOLE subprofession of community medicine (any more than it was all health economists, or medical statisticians) who saw one or other "side" of the unemployment and health debate as a suitable testing ground for their claims. Within community medicine, the majority were probably of a more "managerial" than "public-health" orientation. In any case, they could rest assured that the posts of District and Regional Medical Officers and of "specialist" in community medicine could only be filled by medically qualified persons. Establishing (or re-establishing) a public-health role for the subprofession was therefore not seen as an essential activity by the majority. However, by late 1987, the Acheson Report on the future of community medicine and the public health function was awaited with some anxiety, and the feeling was spreading that previous attitudes may have been too complacent, and that the Acheson Report could "wipe out" community medicine in its present form.

By early 1987, the sociologists and "manifesto" community physicians had found other issues through which to pursue their aims (some of these around a revived debate on the causes of and remedies for class inequalities in health). They more or less withdrew from the debate, leaving the social statisticians of the LS team with their painstakingly established "structural" theory of the effects of unemployment on health which was now, for the time being at least, not a necessary resource either to pressure groups, political parties, professionalising groups, officials, or the decision-makers within research funding bodies. As a result, the knowledge claim that "the absence of 'wearing-off of the unhealthy nonworker effect' supports a structural rather than an individual interpretation of health inequalities" remained a fragile object, little understood, and hardly used in public debate (see Hansard 23 October 1987, col 1046 for the first, rather vague, appearance of the argument in Parliament). It is argued here that the fragility of this argument is the CONSEQUENCE of the strategic re-grouping of those involved in the unemployment and health debate. Certainly there is no evidence to support the opposite case (that the claim was "dropped" by the rest of the policy community because "conclusive" evidence against it had been accumulating). The idea was not "discredited". It was merely abandoned, left in the twilight

world or "limbo" phase of Downs' model of the social-problem process.

McCarthy (1986), in his study of the Child Poverty Action Group (see section 1.2), has made illuminating use of Downs' "stages". He suggests three reasons why social-problem processes enter the "fourth stage" of a "twilight world of lesser attention and spasmodic recurrences of interest."

1. The fact that "some people think the issue is too big to solve"
2. The social problem comes to be seen as confined to the "rough" section of the working class, partly because of media treatment of the issue, and is therefore abandoned by labour organisations
3. The "dilettantism" of "those who simply become bored and disenchanted by the issue and passively await the arrival of a new ... issue". Here "those" include Ministers, civil servants, academics and journalists, as well as "middle class do-gooders" who "flippantly desert" issues, leaving them "largely unresolved" (pp. 100-104)

By mid-1987, all three of these things seemed to have happened to the unemployment and health debate. Civil servants had long regarded the "problem" as "too big", either inherently ("what could the DHSS **do** about it?"), or because it crossed departmental boundaries (see section 3.3), or because of government policy ("You know and I know this government is not going to make a U-turn...", see section 7.4). Seaton (1986, p. 19) found that as unemployment increased during the 1980s, its news value tended to fall. Her study of coverage in The Times showed an "inverse relationship between the percentage of the population out of work and the front page attention given to the subject ...". One journalist, commenting on the failure of the Labour Party to take up Steve Platt's research in 1983, remarked that unemployment and suicidal behaviour was "too big" for a political party as well. Opposition politicians, Bryan Christie felt, look for issues over which they can quickly demonstrate the superiority of their own policies. The spirit in which the likelihood of the existence of "the unemployment effect" was admitted by a policy adviser in late 1984 had been almost one of breathlessness at the enormity of what it entailed (see section 7.4). This was also reflected in a Lancet leading article of November 1984, which, perhaps unintentionally, highlighted the contrast between the (relatively) minute sums that could be spent on even the most expensive research, and what would be necessary to tackle the underlying problem of mass unemployment. Those who dealt with this larger problem in terms of practical economics had never joined in the debate on "health" (see section 4.3.3).

The second cause to which McCarthy attributes the languishing of a social problem process, that is, of the better-off and poorer sections of the working class being turned against each other, amounts to what I have termed "moral fragmentation". In McCarthy's case study, the problem is that of poverty rather than "the health of the unemployed", but the same analysis can be applied. For McCarthy, the media play a decisive role in individualising social problems. Seaton (1986, p. 28) disagrees, and holds that the media "have not been an overriding independent influence on attitudes towards unemployment" but rather acted as a "catalyst" of new attitudes which emerged, as she sees it, mysteriously. In the present case study also, other factors seem to have been at work. The media occasionally picked up statements made by those who opposed the idea that unemployment affected health ("Dole is like a holiday, says economist"). But "Death on the dole" headlines were the vast majority. We need to look elsewhere for the source of individualisation, and this study has argued that it was the entrepreneurial activities of some economists, and the established alliances and new enrolments which they sought, which produced an account of the poor health of the unemployed as a product of individual characteristics. Changes in public awareness and perception of the phenomenon of unemployment in Britain may well have played a role in the easy acceptance of these experts' views, however. By 1983-4, the pattern of unemployment was one of **increasing** inequality of distribution: that is, the risk of unemployment was, if anything, more concentrated within social classes IV and V than it had been in the mid-1970s (Sinfield 1987). There were grounds for thinking that the tendency described by Stern for the 1970s (Stern 1979) had become even stronger as unemployment rose and then stabilised at new heights. As more of the total amount of unemployment was experienced by the same people, either as long-term spells without work or as a life "in and out of work", those in steady jobs had every reason to feel less personally preoccupied by what unemployment might do to **them** if it struck (see section 6.1.2). As these trends became clearer, it was less and less likely that labour organisations would ever be "enrolled" in a debate on the health of the unemployed (although they began to take up the question of the pay and conditions of workers in the "secondary labour market") At the same time, sociologists were showing that there was a growth in the proportion of the labour force employed in low-paid, part-time, and/or intermittent forms of work (Fevre 1986, Walker, Noble and Westergaard 1985, Harris, Lee and Morris 1985, Martin 1987) Unions organising unskilled and service sector worker, as well as



certain industrial sociologists, could have been "enrolled" by those researchers who believed there were important health consequences to recession, and who wrote of "residualisation" as did Fox (1986 unpublished), Moser et. al.(1987), and Platt (1986)<sup>48</sup>.

This unfulfilled possibility is the reason why McCarthy's third point, on "dilettantism", needs to be considered rather more carefully as it applies to the present case study. Firstly, the considerations of "time", priorities, and funding which have been discussed in section 1. 4., indicate that we can perhaps go beyond a concept of "dilettantism" in explaining why expert groups abandon social problems. Academics, for example, must enrol resource holders if they wish to acquire the means to do research of any kind. Secondly, although necessary discretion of both academics funded by government departments and of civil servants created considerable problems in interpreting the interview and field material in the present study, questions about the degree of "pressure" applied to unruly experts have to be addressed. Only a fine line divides the ability to "see which way the political wind is blowing" from "government suppression of research", and some field material did indicate that certain researchers felt under considerable pressure to abandon work on the effects of social inequality and unemployment on health. The spirit of routinely "moving on to the next thing" was certainly present<sup>49</sup> in some researchers. Others, however, by 1987, did seem discouraged even to the extent of feeling a sense of "depression" and futility, which, they feared, had affected their academic "productivity".

There were some, certainly, who felt that academics (in medical and other disciplines) who became involved in the debate had been engaged on an imperialistic exercise, and who might therefore regard their loss of interest as a form of opportunism (such as that attributed to medical sociologists by Strong, 1979) or "dilettantism". This would, however, be rather a simplified picture of the way in which entrepreneurial sub-professions dealt with unemployment and health. "Manifesto" community physicians such as Peter Draper had always

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<sup>48</sup> But the difficulties involved in sustaining any form of "enrolment" between reforming groups and the "unskilled" working class, the unemployed, or other "consumers" of social welfare services has been commented upon, for example, by Hall et. al (1978, p. 91), and Ditch (1986).

<sup>49</sup> though it should be asked why the history of science contains so many instances of problems being abandoned for no immediately obvious reason (see for example Collins and Pinch 1979, p. 239), this questions cannot be addressed here and "moving on" is taken to be a routine practice.

made it clear that the topic was not central to them but only one issue which could be used as a vehicle to promote wider objectives (section 4.1.1). Civil servants (including professional advisers) and journalists, as has been discussed, have a "cycle of credibility" which involves "having a bright idea and fighting for it", and then "re-investing" the career capital thus obtained. If there is dillettantism or opportunism, it must be regarded as institutional in nature. The decline of interest amongst subprofessional factions, for example within medicine, social work, nursing, and health education did, however, have an important effect on the academic debate. This interest had reached its peak in 1981-2, and been reflected in the rush of conferences on unemployment and health which had encouraged groups such as the UHSG and SCUF, and produced a high demand for their expertise. Since that time, however, other events had intervened to increase the confidence with which these professionalising groups could enter new territories. During 1981-2, to judge from the content of their Annual Conferences, health visitors, and even librarians, for example, were adopting the welfare of the unemployed as a legitimate area for the exercise of their skills. But by 1986 it was no longer necessary for these groups to enter into debates over the KNOWLEDGE-claim that unemployment damaged health. The knowledge claim was either sufficiently accepted by decision-makers within the professions and their organisational contexts, or far less important as they found new reasons to claim a portion in the wider social problem of unemployment. As the subprofessional groups stopped raising the issue, journalists were no longer receiving "information subsidy" on the subject. And it was the disappearance of this phenomenon -- sub-professional activism combined with information subsidy -- which lowered the political temperature of the academic debate. No longer a "political hot potato", unemployment and health no longer attracted resources on the "customer-contractor" or "concordat" (see Ch. 3) principles by which government Departments could persuade scientifically respectable groups to devote resources to "fuzzy" topics.

## **10.2 Implications for theory and method**

In attempting to understand the progress of a policy-related academic controversy, and thereby to better understand the shifting status of the factual claims which it may produce, the concepts and methods of the "strong

programme" in the sociology of scientific knowledge have proved valuable. By giving equal weight to all knowledge claims, devoting equal attention to those regarded as "true" and "false", and seeking the social and organisational patterns which give rise to both "rational" and "irrational" beliefs this method allows a common perspective to be applied to studying the actions of "researchers" and "policy actors" ("Authorities and Partisans" in the words of Hall et. al.). An established "fact" in a policy-related inquiry is just one possible outcome of a social problem process. "Facts" occur when statements become an "obligatory points of passage" that bind together a variety of those groups which have entered the process seeking their own different objectives. Recent studies of policy communities (for example, McMahon et. al. 1983) and their interaction with researchers offer a very similar analysis to those of the sociologists of science whose work has been used as a guide in this study. For example, speaking of social research in government, Walker (1987) feels that

...it would be wrong to underestimate the individual motives and sub-departmental interests that underpin some government research. In the same way that researchers may initiate research to further their own individual and collective interest, so do policy makers. ... As a result, policies tend to evolve -- in part at least -- through a process of conflict and changing alliances between policy divisions in which research is one of the many 'weapons' that are deployed

Describing the wider context of such Whitehall dealings, Moon and Richardson point out that:

Virtually all political issues carry with them a constellation of groups, jockeying to influence policy-makers ... and trying to influence the definition of the issue to their own advantage.

Groups are engaged in pursuing a wide variety of objectives, and "knowledge-claims" are a currency (amongst others) in this process. A knowledge claim which proves useful to the all relevant groups becomes an "obligatory point of passage" and is accepted as "fact". Scientific debates will persist when unadjudicated claims create "bottle-necks" which hinder either the accomplishment of professional or political objectives (which, as we have seen, are often closely inter-related). But debates do not only end in "closure" by the establishment of "facts". They can also end when the participants simply disengage, when other means of reaching groups' objectives become available. Under these circumstances potential points of passage become mere diversions

or dead ends, and knowledge claims no longer attract even enough attention to sustain controversy.

We may think of other examples of (at least temporarily) abandoned knowledge claims, such as the outcomes of debate on race or class and intelligence (Harwood 1979, 1982), and of the investigation of environmental hazards such as lead or radiation. What is the current epistemological status of the factual claims made in the course of these debates? Sociologically, the question is not whether some are true and others false, but rather what were the organisational and political circumstances which gave rise to these claims and what changes in these circumstances led to the ending of controversy? This may result from the judicial decision at the end of an inquiry, for example, or a re-organisation of the school system brought about following a change of government. Such questions are pertinent (according to the "rule of symmetry" discussed in section 2.1) whether or not one aspect of this "closure" of debate was an acceptance of the truth of specific knowledge-claims.

At the outset of the present study there were two aims:

1. To follow a policy-related academic debate "in hot blood", to see whether what some onlookers and participants (including myself) saw at the time (late 1982) as academically superior research would have a greater impact on policy discussion and decision-making than the "discredited" work of M H Brenner.
2. To attempt to apply theoretical approaches drawn from sociology and social policy (the "social problems perspective") and from the sociology of scientific knowledge, and specifically the "translation model", to the understanding of the debate as it progressed.

The present account has been structured by the attempt to apply a social-problems perspective elaborated according to the "translation" model of the construction of scientific knowledge. Material has been selected by a series of decisions taken on the basis of the theoretical approaches adopted. These approaches, however, were not adopted at the beginning of the study and carried throughout. I have included, in chapters 1. and 2., some discussion of the ways in which theoretical ideas were tentatively applied and, in many cases, had to be rejected, sometimes after a lengthy process involving the collection of much material, which ended up as one or two sentences. For example, I felt that the prediction that media interest in the health aspects of unemployment would be aroused by spates of factory closure stories, marches of the unemployed, or other similar "macro-social" events could be regarded as

discredited only after some two years of collecting newspaper cuttings, pressure-group minutes, and notes from Hansard. This finding increased my confidence in the idea that the social-problem process in this case was created by entrepreneurial activities amongst subprofessional and subdisciplinary groups. But to see it as a "positive" finding IN SUPPORT of my analysis is to ignore my earlier "hunches" about the role played by "the media reacting to social movements and macro-economic change" as a force in the social-problem process. The reluctance of the media to follow up plant closures with "health" stories appeared, at first, as something as puzzling as the insistence of a high proportion of my scientists that they were totally uninterested in the health effects of unemployment full stop, and even moreso in whatever "political" meaning the debate might be seen as having.

Nor did I set out with a conviction that the two approaches I adopted would converge into a common perspective (although I began to hope so in a very vague way quite early on). At first, the two approaches (the social problems perspective and the "translation" model drawn from sociology of scientific knowledge) appeared to apply to quite separate aspects of the debate. Interview and field material relevant to the scientific controversy was addressed with the aid of concepts drawn from the "translation" model as described in Chapter 1. The social-problem "stages" model was used to make sense of the policy debate and the material derived from interviews with officials, activists, and media-workers. As both the history of the debate and the progress of my interviewing continued, however, there were two developments.

Firstly, it began to be clear that there were more similarities than I had expected between the ways in which members of the different groups involved in the debate spoke about their own actions. Secondly, a new literature began to emerge in the sociology of scientific knowledge, which seemed to point to a convergence between ways of understanding "pure" scientists, and ways of understanding "technologists", administrators, and even "politicians". Sociologists of scientific *knowledge* (this is the point of not calling the perspective "sociology of *science*") had long insisted that the cognitive closure of scientific debates could only be accounted for in terms of the social relationships within which science is located. My own struggles with the data in the present study, which stubbornly refused to display social or political "interests" as characteristics differentiating the antagonists in the debate, led

me to concentrate on the professional rather than the political allegiance of participants, and on the micro-politics of what seemed to be the relevant "technology", namely, "social engineering", the technologies of welfare.

In conclusion, to return to the set of questions of theory and method posed at the beginning of this account, can approaches derived from the sociology of scientific knowledge and a "social problem" perspective help to organise the material produced in a case study such as the present one in such a way as to aid in understanding its progress?

A "stages model", and Spector and Kitsuse's method of "following the problem wherever it goes" both guides the researcher through the debate and helps to order the material which results. However, as Manning has pointed out, the "stages model" requires to be adapted and elaborated by a more detailed understanding of the different groups which become involved in social problem processes. In the present study this has involved drawing concepts from other work, such as Gandy's on the media, Richardson and Jordan, Heclo and Wildavsky, and Lindblom on the operation of "issue communities", and sociologists of scientific knowledge on science as a socially organised activity. In the process, it has emerged that scientists, journalists, professionals and officials can all be regarded as following the "cycles of credibility" appropriate to their occupational cultures. In order to understand WHO becomes involved in policy-related academic debates, these aspects of occupational culture need to be addressed. In order to understand what those who become involve **do**, it is necessary to trace out the objectives of the groups involved, and the ways in which individuals and groups pursue these objectives by "inter-esting" and forming alliances with each other. In this sense, it does seem that "interests" play an important role in the framing, contestation, and acceptance of knowledge claims as "fact". However, in the present study, neither social background nor political allegiance seemed to account fully for participants' actions. In addition, it was necessary to understand the "interest-work" carried out by individuals as members of entrepreneurial sub-professional or sub-disciplinary groups, and the relationships of these groups to each other and to other, often similarly "entrepreneurial" clientgroups.

The social problem perspective not only provided a useful guide to the overall progress of the debate (the "stages"), but also clarified aspects of the scientific process WITHIN the debate. In fact, the "translation" approach itself

can usefully be regarded as a type of social-problem perspective.

The "translation" approach was found to be a satisfactory guide, not only to understanding the actions of scientists, but also of officials and professionals, both in the "more" and "less academic" phases of the debate. This might have been expected, given that the chosen debate was one with a high "scientific" content. However, as outlined above, the translation approach does benefit from insights drawn from the social-problems perspective, and needs to be further developed by other case studies involving more detailed consideration of the dynamics at work in the "trans-scientific field" of government departments, funding bodies, professions and wider social movements.

How well did concepts taken from these two perspectives "fit" the unfolding events in the debate?

1. "Moral and technical fragmentation", as described by Manning, did seem to take place. However, once again events seemed to call for a rather more complex application of the concepts than proposed
  - a. "Technical fragmentation" was seen to be a response to strong contestation of a political and/or academic type (or, more accurately, the to the type of powerful academic counter-argument which could be produced given the policy-decision to "interest" academics in investing time and effort in such an argument.) Rather differently to what is proposed by Habermas (1971, and see also discussion of this by Scott 1988) and some of his followers, the present study did not find that technical fragmentation operated as a form of social control exerted by or through scientists. On the contrary, the more technically fragmented an argument became, the more likely it was that all sides of a policy debate would eventually lose interest in it.
  - b. "Moral fragmentation" could take place at higher or lower levels of technical sophistication and was independent of this. Moral fragmentation comes closer to the concepts used both by Habermas and by Manning, that is, the "reduction" of a collective problem to one concerned with the characteristics of individuals.
2. The role of the media and the concept of "information subsidy": The role of the media was not at all what had been expected at the outset of the study. Rather than any consistent tendency of journalists to "*independently*" distort or sensationalise research results and other forms of expert pronouncement, it appeared that experts' attempts to "interest" client groups and to claim a role in the social problem process played a major role, through the mechanism of information subsidy, in producing media "stories" which attracted public attention.
3. The role of "*pressure groups*", in its turn, appeared far more similar to that of entrepreneurial "ginger groups" within professions and disciplines than had been expected. The first hint of this was the difficulty I had when coding my interviews, deciding who to code as "P" (professional) and who as "A" ("activist"). The initial distinction between "authorities" and "partisans" (Hall, Land, Webb and Parker) was far too simple to be

maintained throughout the case study. For one thing, the most energetic members of pressure groups were also enterprising professionals or academics, and NOT (as may be the case in other social-problem processes) members of wider social movements.

Concepts and rules of method derived from recent work in the sociology of scientific knowledge have guided the analysis of the activities of individual members of the debate's "core group" and of the professional and disciplinary factions to which they belonged.

1. The two principles of "beginning with controversy" and "following the course of action wherever it leads" are common to both Latour and Callon's translation perspective and Spector and Kitsuse's method for the investigation of social problem processes. Adherence to these rules of method was more or less inevitable given the starting point of the present study.
2. The "principle of symmetry" helped to avoid attempts to "fix" the state of knowledge in the debate at any given point in time as having established what was "really the case", which would have been a fruitless exercise and might have cut off the investigation arbitrarily.
3. "Explanation by association and situation" has helped to concentrate the analysis on the shifting balance of alliances between scientists, professionals and officials which gave the debate its shape.

We have seen in this account the varying degrees to which subdisciplines concentrated on "translating" their theories, methods and findings into forms which would attract allies from their trans-scientific fields, and the outcome of these efforts in terms of research coming to be seen as more or less "policy-relevant", "fundable" and "yielding firm results". One aspect of the enrolment process made more visible in the present case-study than it may have been in some others (for example, Callon 1986, Coutouzis and Latour 1985) is the effect of science policies formulated at governmental level and acting via decisions made in the "independent" research councils. The effect of the Rothschild reforms in encouraging research councils to look favourably upon "policy-relevant" work was to raise the priority of "unemployment and health" in the eyes of MRC-funded research teams. As well as making it "controversial", the political salience of the debate also made it "policy-relevant". This had consequences for scientists' opportunities to advance their own individual "cycles of credibility" and those of the research groups they belonged to (see section 7.2).

As Collins (1985) has warned, however, attracting more attention is a



double-edged weapon to a research team. New allies (as Latour 1987 also points out) can be unreliable, and new opponents may also be attracted into the field. However, as Shapin (1982) has argued, conflict also intensifies scientists' efforts to construct their claims as "objective" and "value-free". The knowledge-claim "the effect of unemployment on mortality cannot be due to selection" was based on an intricate argument. This argument expressed a tentative "lash-up" of several professional and disciplinary subgroups: public-health oriented epidemiologists and community physicians, occupational medicine, vital statisticians concerned with "social indicators"<sup>50</sup>. Whether this statement would become a "fact" depended (and continued to depend at the end of the study period) on the strength of the associations and alliances around the researchers. Between September 1986 and late 1987 these alliances appeared to become "untied", and the "wearing off of selection" began to fade towards uncertainty. But it does not by any means follow that the statement will not, at some future time, regain a degree of "solidity", if it once again provides a potential "point of passage" through which a sufficient number of interest-groups must pass in pursuit of their aims, and if these aims are met with "success" in policy terms. In the unlikely event, for example, of a marked equalisation of living and working conditions in British society being followed by a reduction in class inequalities in mortality, the ideas developed at the Social Statistics Research Unit might well come to be regarded as unquestioningly as those of John Snow on cholera (Jones and Cameron 1983) or Pasteur on anthrax (Latour 1984b). It is, however, as pointed out by the economist in section 7.4, most unlikely that such policy change would come about BECAUSE the arguments of any particular group of researchers were "accepted"<sup>51</sup>.

This argument returns us to the question of the relationship between the "quality" of research (as judged by the "scientific community") and its impact on policy debate. Bulmer (1986) points out that in Cohen and Garet's (1975) study of the effect of applied social research on educational policy-making:

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<sup>50</sup> Another group which might have been expected to be included was the "poverty lobby", but by late 1987 this enrolment had not yet been seriously attempted from either side (researchers' or "activists'") perhaps for the sorts of reasons discussed in the context of the Stirling conference of June 1982 (see section 4.3.3).

<sup>51</sup> Although an important and rather unexpected postscript to this account occurred in February of 1988, when John Fox was appointed Chief Medical Statistician at the OPCS.

There was no clear connection between relevance, methodological sophistication and authoritativeness [partly because] methodologically superior knowledge was more complex, arcane and hard to interpret (Bulmer 1986)

In the course of the debate described by Cohen and Garet, as in the present case study

the knowledge produced improved by any scientific standard, but was not more authoritative by any political standard, and often more mystifying by any public standard (Bulmer 1986, p. 26–27).

However, it cannot be concluded from this study of the unemployment and health debate that there is necessarily a trade-off between “quality=complexity” on the one hand and “clarity=over-simplification” on the other. Brenner’s work was regarded as the most “arcane” by participants, though not as the “best”. Nor can the failure of the LS team’s work to become widely accepted be attributed to its complexity alone. Highly technical work, in this debate, was sometimes seen to be relatively easily translatable, and less technical work more difficult to communicate to participants in the policy process.

The perspectives used in this study should perhaps lead to a re-formulation of questions such as

- “What is the influence of research on policy?”, or
- “Is the lack of influence of social research on policy due to the poor quality of the research?”

It might be more fruitful to inquire into the ways in which “social problem processes” produce opportunities for claims to expertise to be made, and thereby shape both “debated” and “accepted” knowledge itself. Such a reformulation has been suggested by Rein, who sees the relationship as an “interplay” (Rein 1980) and holds that

intervention pushes research rather than the other way around ... The challenge is not linking research to policy, but uncovering the latent policies that organize the empirical research carried out by social science (Rein 1983, p. 228, 245).

Such a reformulation would bring us closer to the ways in which participants in policy-related academic debates themselves see the relationship -- as constructed within an iterative process in which individuals and groups make claims about the “truth” and “policy-relevance” of some aspect of their

work as a form of strategic advance into a new field or market-place which has opened up as a result of conflict and change in the policy making arena. Claims-making is therefore adapted to the different perceived characteristics of the field, such as the nature and strength of the opposition, and the "interests" of potential allies. Knowledge claims weave their way in and out of policy debates, often by means of such "vectors" as researchers' tactical use of information subsidy, and participation in pressure groups. Other tactics (less visible to an outside investigator and therefore not discussed at length here) include informal contact with decision-makers, as evidenced by officials' nervousness about "stirring-up" members of the academic community who are seen as able to mobilise either the media or "powerful acquaintances" with the ability to influence funding decisions. Future case studies may examine the fate of other knowledge-claims in relation to their articulation with policy objectives in this more "symmetrical" fashion.

Outcomes of the interactions between the parties to a debate could also be seen in terms of quite transparently "political" objectives: "Ministers didn't want to know". However, both the present case study, and much existing literature on British policy-making shows that (unlike many of the participants in the debate), the sociological analyst should not simply assume that a social-democratic government is sympathetic to research and a conservative one less so. McCarthy (1986) argues that the "right-wing research institutes" had a strong influence on Conservative economic and social policy, by virtue of persistent and painstaking lobbying for certain ideas over a long period of time. As we have seen, knowledge claims are produced and elaborated by individuals and groups with their OWN aims and objectives, not reactive in any simple sense to "political pressures". Understanding political allegiances seems to be less helpful than understanding the ways in which expert groups enter policy debates with a history of participation in previous social-problem processes, which has established their methods and the data available to them as "resources". At the time of the debate, work within government departments provided a major market for the skills of both economists and statisticians. However, the work of the economists may be regarded as including a form of "management of an economy". That of statisticians, in contrast, was more in the nature of "management of a population". The different technologies involved in these two different kinds of management both influenced and constrained what could or would be adopted and offered to client groups as

either a "problem" or a "solution". Put somewhat crudely, there was a knowledge-claim:

- "Inequality" (social or medical) is a result of the "unfitness" of a small section of the population. This unfitness is either inherited or acquired in early childhood (due to poor parental skills). Therefore, differences in health or life expectancy between groups with different experiences of *adult* life should not be attributed to those experiences (work, income, unemployment).

which had been proposed by various researchers over a very long period of time (for an account see Jones 1986). It was reflected, for example, in Illsley's much-respected work on perinatal mortality carried out in the 1950s. Jon Stern's work on health inequality described in chapter 6 could be seen (as it was by Fox, for example) as located in the old Eugenic tradition discussed by Mackenzie. This knowledge-claim came to be seen as a potential resource by policy-makers who took a counter-Keynesian stance. A good early example of this was Sir Keith Joseph's notion of "transmitted deprivation". An opportunity for "enrolment" was therefore presented to economists working with ideas of a "human capital production function". Although many economists would not regard it this way, there is a possible translation of "human capital theory", espoused at least transitorily by several economists prominent in both the unemployment and health and the health inequality debates (Stern, Gravelle, see also the references given above to the work of Julian le Grand and Illsley himself on health inequality), which allows the theory to attribute misfortune to individual characteristics and "free choices" (for example, to invest in education, exercise or a healthy diet). Individual economists may later have abandoned this interpretation, but during the most politically heated period of the unemployment and health debate, much of the evidence presented here (for example, sections 6.2.1, 7.4) seems to indicate its importance.

Whether or not the enrolment between certain economists and policy-makers would hold remained to be seen by the end of this case study. The work of the LS team presented a potential challenge to such an alliance because they were attempting to establish the concept of the "wearing off of selection" as a method of distinguishing between group-specific patterns of mortality which could be attributed to selection (pre-existing "unfitness") and those which could not. But knowledge claims about the effects of unemployment and class inequality on health did not DETERMINE the outcome of these attempts. On the contrary, the truth-status of the various "findings"

and "models" was a RESULT of the varying success of the groups that made them in enrolling allies and maintaining their alliances. An impression was growing by the end of field work that the threat to the size of the market for the economists' skills posed by successive waves of government re-organisations was making the enrolment between economists and policy-makers (in this and in other issue communities) increasingly tenuous. Booth (1982), for example, has observed that:

Social planning is in the doldrums ... After all, who needs to bother with planning if the market, released from the distortions and burdens of state intervention, official regulation and bureaucratic red tape, will by itself maximize national wellbeing?

And Michael Heseltine, when Secretary of State for the environment, was reputed (by disconsolate government statisticians) to have said, "Research on housing? We don't need research, we take decisions on housing and we implement them, that's all there is to it". Such trends in government thinking were more likely to encourage an alliance between the expert groups themselves.

One criterion by which to judge the usefulness of the present study would be whether it could equip other researchers better to address questions about other teams of experts, such as the "right-wing" Institutes (Adam Smith, Economic Affairs) and the "non-aligned" ones (Brookings, PSI), and their relationships to political parties and to Whitehall, Westminster, or Capitol Hill<sup>52</sup>. The origins of the unemployment and health debate can be seen, on one level, as residing in the much grander conflict in which counter-Keynesian economic thinkers sought to mobilise behind their technical claims (in the form of "scrounger debates" for example), a growing social movement, that is, the growing discontent with the failures of mixed economy "welfarism"<sup>53</sup>. The outcome of the present study would suggest that future research on the question of the relationship between research and policy might consider more carefully the positions of professional and disciplinary sub-groups as "entrepreneurial" claimants to areas of expertise, and of both political parties

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<sup>52</sup> A most interesting example is recently provided by Anna Pollert's analysis of the concept of the "flexible firm" as a product of the mediating role between government and industry played by an "intermediate body", the Institute of Manpower Studies, see Pollert 1987.

<sup>53</sup> This would account for the attractiveness to some on BOTH sides of the debate of the ideas of M H Brenner, which bore the mark of his early training as an economist in the Keynesian tradition.

and government officials as “brokers” of social–problem areas.

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